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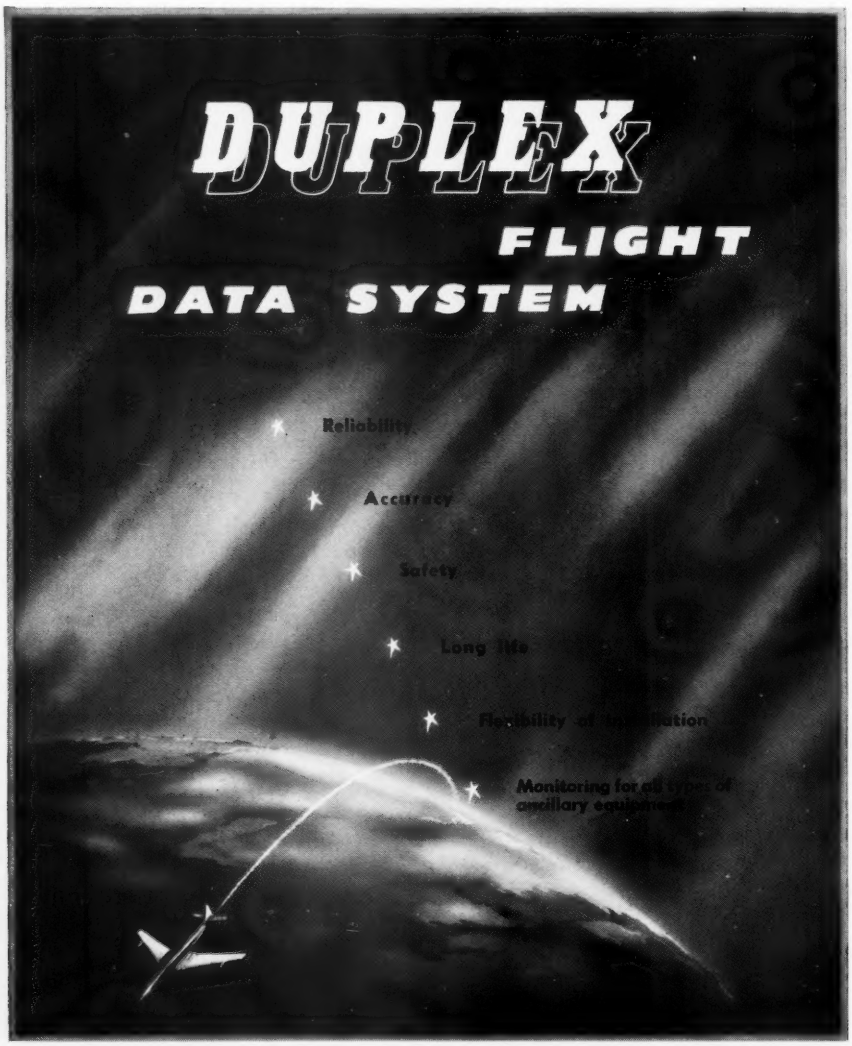

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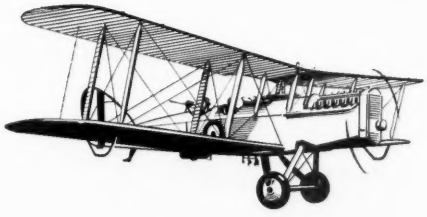

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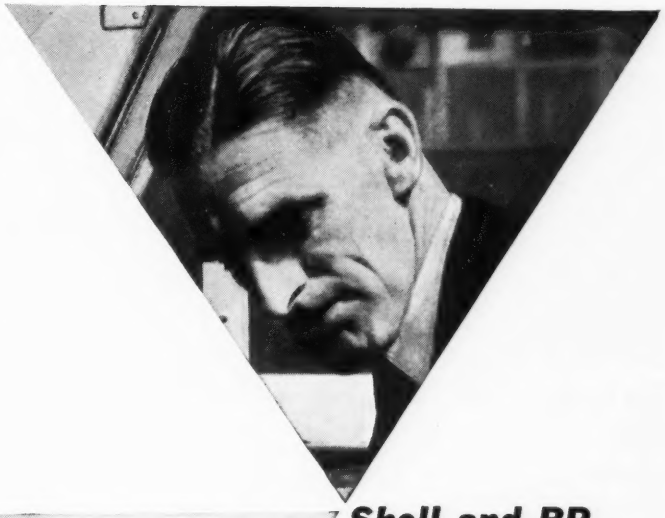

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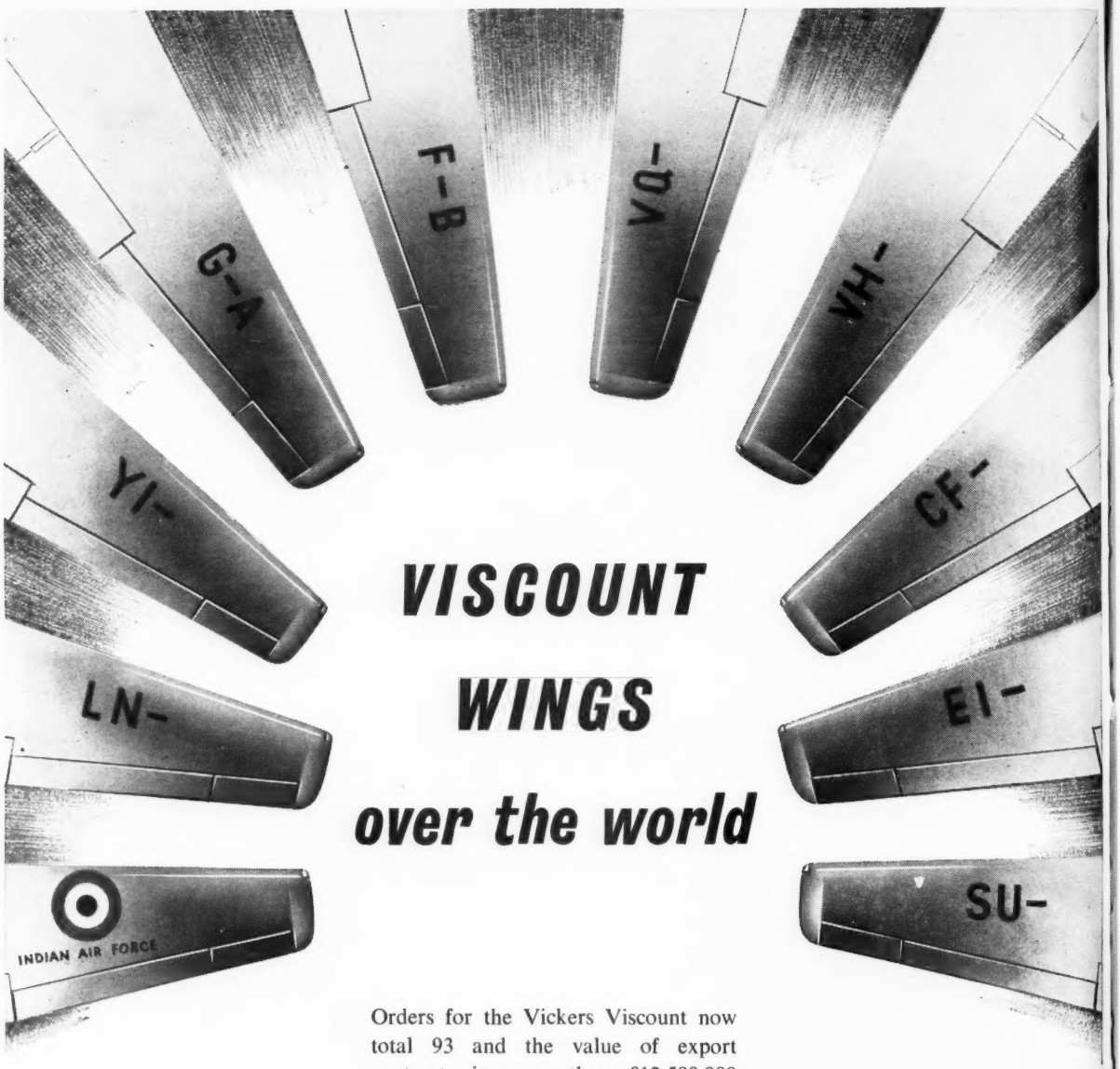

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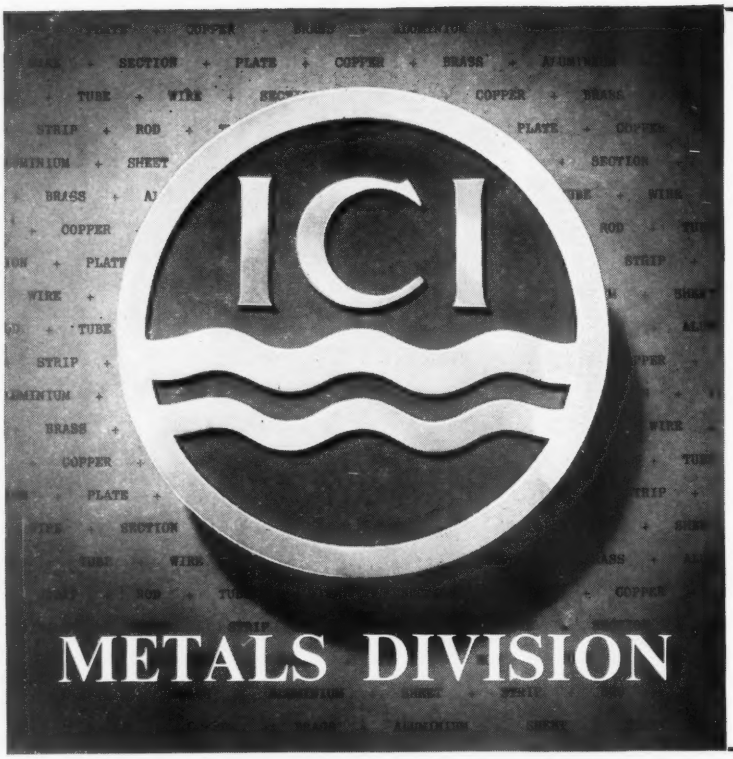

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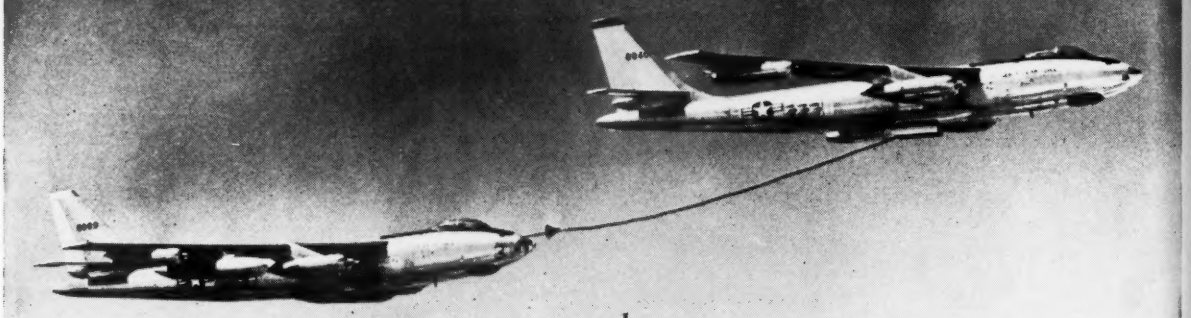

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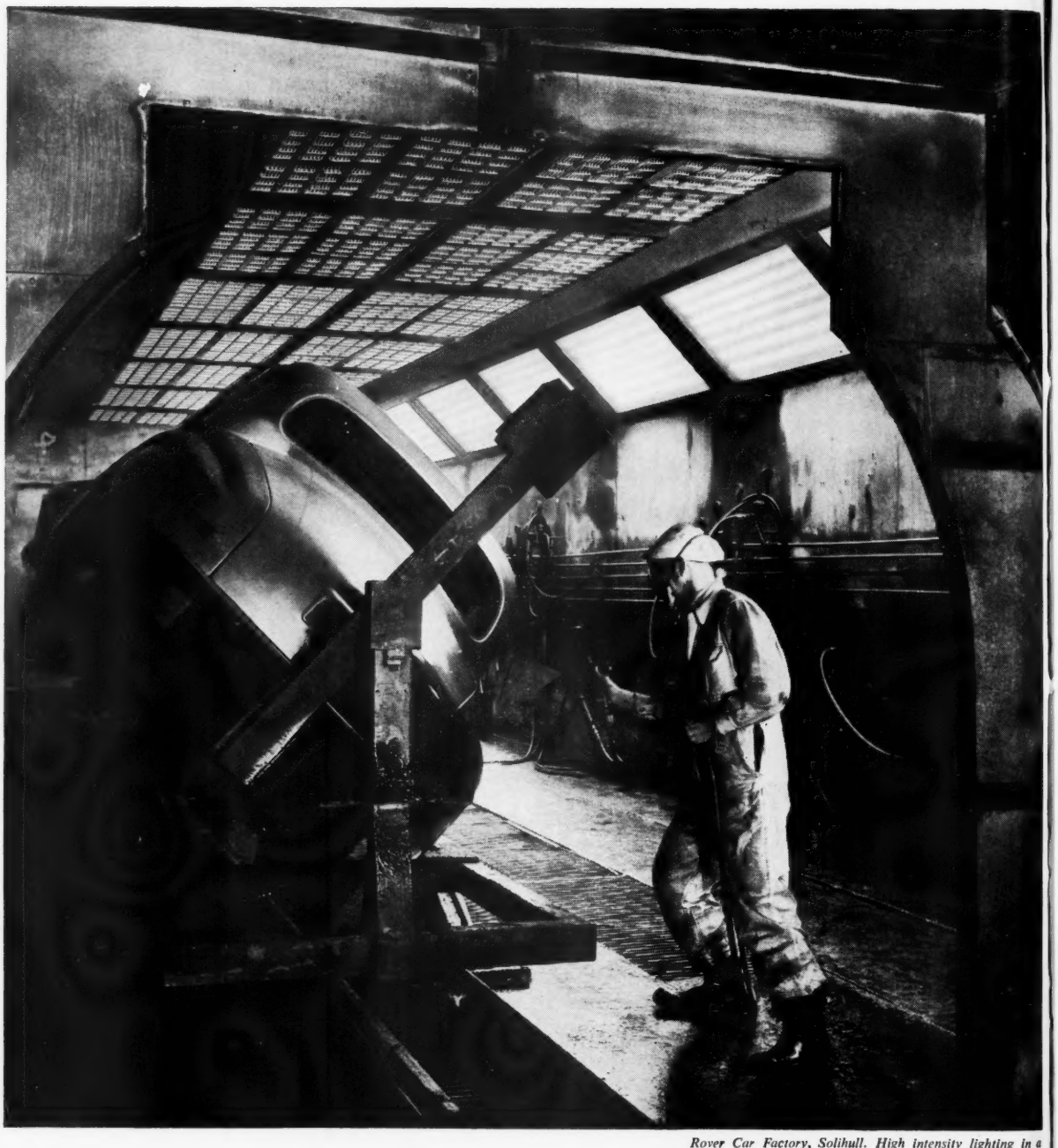

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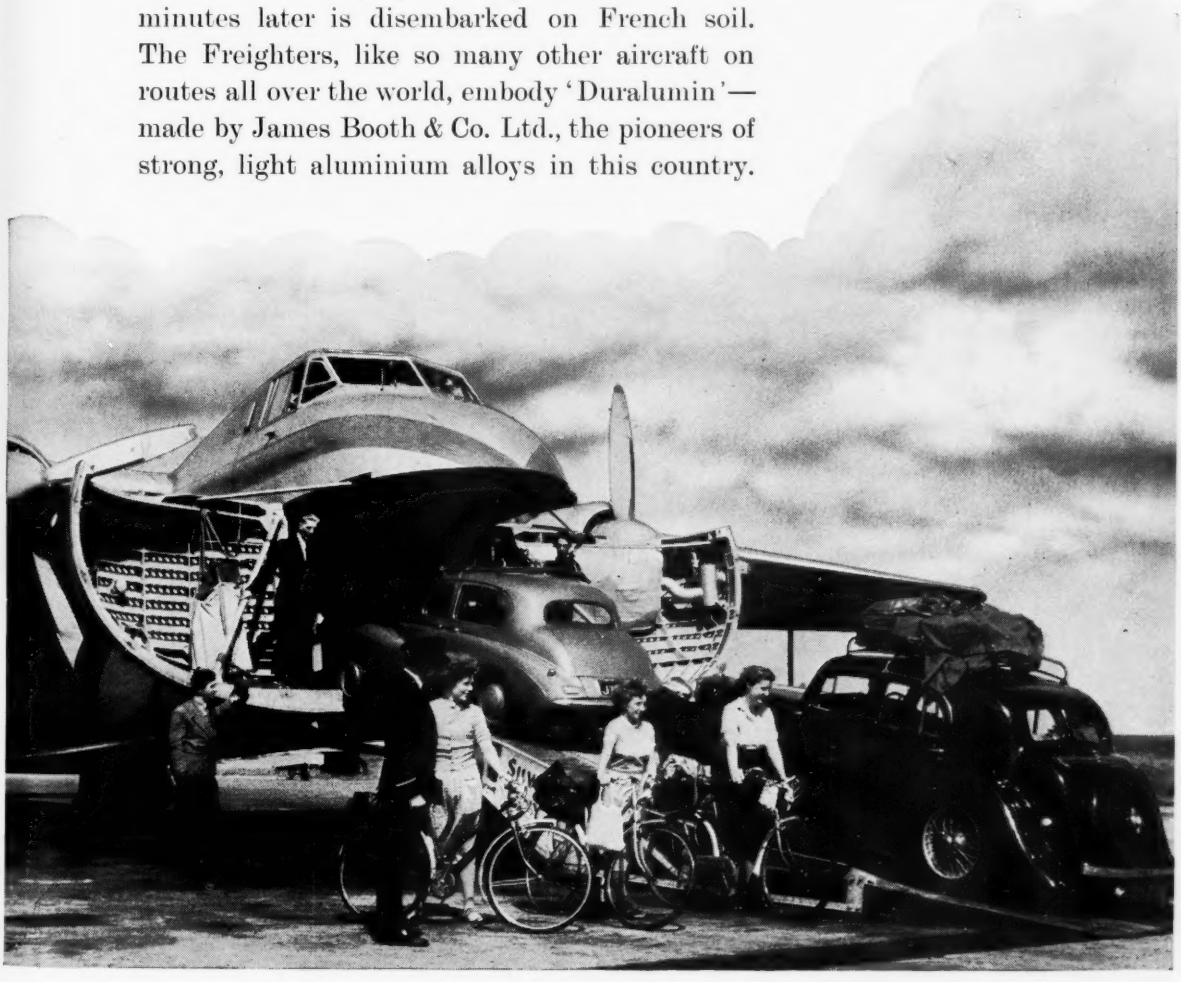

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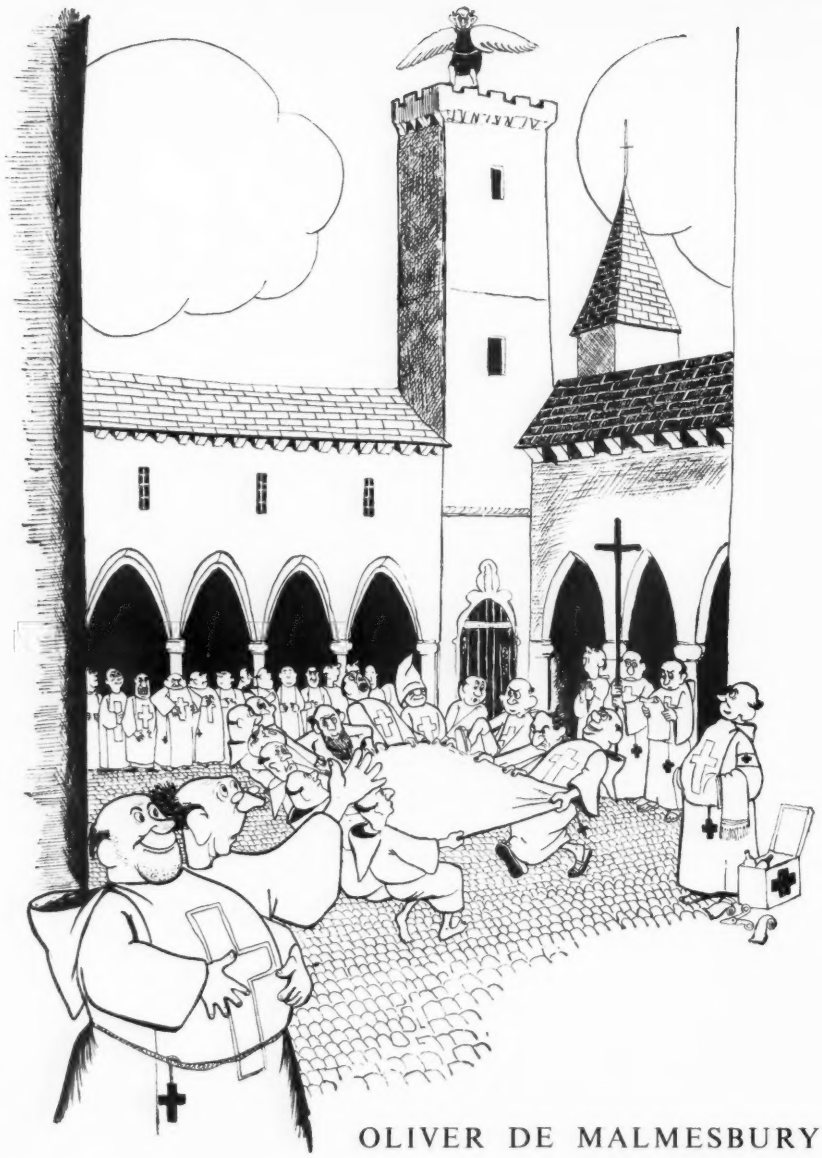

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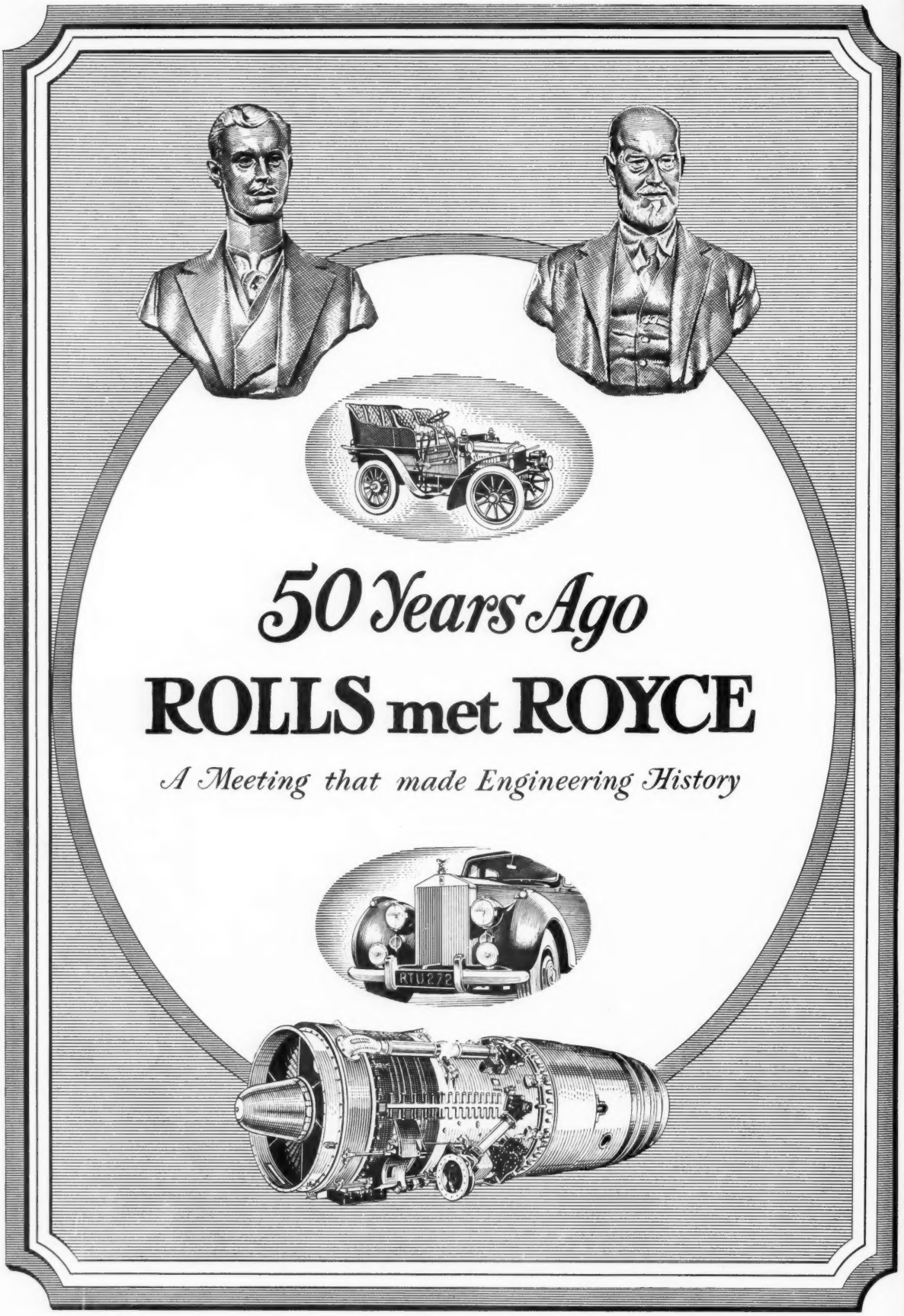
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Chairman: D. B. SMITH, O.B.E., A.F.R.Ae.S.
Hon. Secretary: H. C. SMITH, F.R.Ae.S.,
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YEOVIL
Hon. Secretary: L. A. LANSDOWN, A.F.R.Ae.S.,
Westland Aircraft Ltd., Yeovil.

NOTICES

An annual sum of £250 is available for premium awards for papers, including Technical Notes, published in the Journal. Members and non-members of the Society are invited to submit papers on any aspect of aeronautics

Whitsun Holidays

The Offices and Library of the Society will be closed from 5 p.m. on Friday 4th June to 9 a.m. on Tuesday 8th June 1954, for the Whitsun Holidays.

Closing of the Library

The Library will be closed on Tuesday, Wednesday and Thursday 25th, 26th and 27th May 1954 for the Associate Fellowship Examination. Full particulars have been sent to all candidates.

The 42nd Wilbur Wright Memorial Lecture

The 42nd Wilbur Wright Memorial Lecture will be held on Thursday 20th May 1954 at the Royal Institution, Albemarle Street, London, W.1, at 6 p.m. (Tea at 5.30 p.m.). The Lecture will be given by Dr. A. E. Russell, F.I.A.S., F.R.Ae.S., on "The Choice of Power Units for Civil Aeroplanes."

Technical State Scholarships 1954

Arrangements for the award of 120 Technical State Scholarships to students from Technical Colleges and other Establishments for Further Education will be continued in 1954 on the same general lines as hitherto. Most of the Scholarships will be offered to students who have obtained an Ordinary National Certificate or other qualification of equivalent standard and will enable them to pursue full-time Honours degree courses or courses of equivalent standard at Universities, University Colleges or Establishments for Further Education.

This year again a limited number of awards may also be made to enable suitable candidates who have obtained an initial qualification of University first class degree or Higher National Certificate standard, by part-time work to attend approved post-graduate courses of study or research of an applied or technological nature. The award will be available for a minimum of one year and a maximum of three years.

Forms of application may be obtained from the Principals of Establishments for Further Education or from the Ministry of Education (F.E. 1(b)), Curzon Street, London, W.1.

Graduates' and Students' Section—Visits

A visit has been arranged to Vauxhall Motors Ltd. (Luton, Beds.) on 4th June. Applicants are requested to state their mode of transport, *i.e.* private or public transport, so that party travel can be arranged. Applications should be sent to the Honorary Visits Secretary, D. J. W. Richards at 18 Geneva Road, Kingston, Surrey, as soon as possible.

A visit has also been arranged to the Headquarters of the London Control Zone on Saturday 26th June. Visitors will be able to see the equipment, etc., connected with Air Traffic Control at Uxbridge and London Airport. As numbers are strictly limited members are requested to apply, as soon as possible, to the Honorary Visits Secretary, D. J. W. Richards, 18 Geneva Road, Kingston, Surrey.

Repeat of Section Lecture

The Section Lecture given by Mr. H. L. Cox, M.A., F.R.Ae.S., on "The Use of Materials in the Plastic Range," which was originally delivered on October 13th 1953, will be repeated on Thursday 13th May 1954. The lecture will be held in the Library of the Society at 7 p.m.

Garden Party, Sunday 13th June 1954

The Society's Annual Garden Party will be held in a Special Enclosure at London Airport, by kind permission of the Ministry of Transport and Civil Aviation, and Sir John D'Albiac (Airport Commandant) on Sunday 13th June 1954.

An application form for tickets has been sent to all Home Members, and it is hoped that Members will apply as soon as possible for their tickets, in order to help with the organisation.

The Nash Collection of Veteran Aircraft will be on show for the first time since it was bought by the Society. It is hoped that members will also have an opportunity of inspecting some new air liners.

News of Members

P. R. Allison, M.B.E. (*Associate Fellow*) has been appointed to the newly-created post of Service Manager of Flight Refuelling Ltd. He was previously Senior Experimental Engineer with the company.

H. N. D. Bailey, B.Sc.(Eng.) (*Associate Fellow*) has been appointed Chief Test Pilot to Rolls-Royce Ltd.

J. Barker (*Associate Fellow*) has been appointed Assistant Chief Draughtsman, London Design Office, Aircraft Division of the Bristol Aeroplane Co.

It was recently announced that Sir Harold Roxbee Cox (*Fellow*), Past President of the Society, is to be awarded the Honorary Degree of Doctor of Science by the University of Birmingham.

Sir Roderic Hill (*Fellow*), Rector of the Imperial College of Science and Technology, has been re-elected Vice-Chancellor of London University for the year beginning 1st September 1954.

J. O. Hitchcock (*Associate Fellow*), formerly Assistant Managing Director of Henry Wiggin & Co. Ltd., has been appointed "Assistant to the Chairman" of the Mond Nickel Co. Ltd. He remains a Director of the Wiggin Company.

Wing Commander A. W. Lindley (*Associate Fellow*) will be taking up a post as Technical Author with Rotol Ltd. on his retirement shortly from the R.A.F.

Cyril Luby (*Associate Fellow*), at one time General Manager of Rotol Ltd., has been appointed President of Canadian Steel Improvement Ltd., a new Canadian Subsidiary of the Hawker Siddeley Group.

Peter Masefield (*Fellow*), Chief Executive of B.E.A., has been appointed a Vice-President of the Institute of Transport.

A. O. Mattocks (*Associate Fellow*) has been appointed Works Manager of Saunders-Roe (Anglesey) Ltd. He was previously Production Engineer at Saunders-Roe, Isle of Wight.

Professor A. J. Sutton Pippard (*Fellow*) has been elected a Fellow of the Royal Society.

H. L. Stevens (*Fellow*), who has recently resigned from his post as Principal Director of Equipment, Research and Development (Air) at the Ministry of Supply, has now joined Messrs. Sandberg.

J. D. Stranks (*Associate Fellow*), formerly Chief Experimental Engineer, Hawker Aircraft Ltd., has been elected to the Company's Board as Works Director.

Diary

LONDON

May 13th

SECTION LECTURE. The Use of Materials in the Plastic Range. H. L. Cox. In the Library. 7 p.m.

May 20th

THE 42ND WILBUR WRIGHT MEMORIAL LECTURE. The Choice of Power Units for Civil Aeroplanes. Dr. A. E. Russell. Royal Institution, Albemarle Street, W.1. 6 p.m. (Tea 5.30 p.m.)

BRANCHES

May 12th

Chester.—Annual General Meeting. Grosvenor Hotel, Chester. 7.30 p.m.

Leicester.—The Medical Effect of High Speed and Aerobatic Flying. Squadron Leader R. S. Warrbeck. Leicester College of Technology. 7.15 p.m.

May 14th

Glasgow.—Rocket Propulsion and Interplanetary Flight. A. V. Cleaver. Royal Technical College. 7.30 p.m.

May 19th

Hatfield.—Annual General Meeting and Film Show. de Havilland Restaurant, Hatfield. 6.15 p.m.

INSTITUTE OF THE AERONAUTICAL SCIENCES MEETING

The Institute of the Aeronautical Sciences is holding a meeting on "Turbine Powered Air Transportation," from 9th-11th August in Seattle, Washington. Papers are being submitted by several British authors, as well as American, and a tour of the Boeing Plant and inspection of the Boeing Jet Transport Prototype will be included as part of the Programme. The Institute of the Aeronautical Sciences is most anxious to encourage British participation. Further

ACKNOWLEDGMENTS

The Council acknowledge with thanks the return of back numbers of the JOURNAL from Eric C. Gibbons, Esq., *ex-Assoc. Fellow;* E. W. Roberts, Esq., *Assoc. Fellow;* C. F. Turner, Esq., *Assoc. Fellow;* and S. J. Noel-Brown, *Assoc.*

JOURNAL BINDING

Self-Binder Cases

Self-Binder cases of the "Easibind" type are available from the offices of the Society. These binders are for members who do not have their Journals permanently bound, or who wish to keep their Journals together during the year for binding later.

These cases will hold 12 Journals which are kept in place by means of flexible steel wires. Journals can be inserted or withdrawn easily without damage, so preserving the contents for permanent binding later. The Journals will open flat at any page.

The binder is strongly made in durable dark blue leather cloth on stiff board covers and has gold lettering on the spine. The year is not blocked on the spine but there is a panel on which members who wish to use the binder as a permanent case can put the date.

The cost is 11s. 6d. each including postage and packing for either the size to fit 1952 and previous Journals, or for the size to fit the Journal from January 1953, which has been increased in size. Orders and remittances should be sent direct to the Secretary at the Offices of the Society and *whether the old size or new size is required must be stated.*

Permanent Binding

There is no increase in the price of permanent binding of Journals. The prices are:—

1953 Volume (including packing and postage) 16s. 0d.
Previous Volumes (including packing and postage) 18s. 0d.

Journals, *with a note of the name and address of the sender,* should be sent direct to The Lewes Press, Friars Walk, Lewes, Sussex, and the remittance to the Secretary at the Offices of the Society.

ELECTIONS

The following is a list of new members and transfers of membership of the Society:—

Associate Fellows

Walter Leonard Abigail
John Barker (*from Graduate*)
Brian Arthur Howard Botting (*from Graduate*)
Dirk Willem Bouwer (*from Companion*)
Edward Seth Bradley
Kenneth Patrick Cater
Eric Alan Coates (*from Graduate*)
Peter Bruce Dennis
Frederick George Dummer
Roland Boyd Ferris
Samuel Frank Follett
Georgina Joan Griffith (*from Graduate*)
John Kenneth Haviland (*from Graduate*)
Robert Lee Hayes (*from Associate*)
Michael Alrick Josselyn Morton Hayward (*from Associate*)
Derek Vance Hilborne (*from Graduate*)
Alan George Hobbs (*from Graduate*)
Hall Pringle Sigley Hogg
Edward Lewis Houghton (*from Graduate*)
William Kuzyk
Jack Allen Packett (*from Graduate*)
Derek Gordon Page (*from Graduate*)
Peter Alfred Henry Pooley (*from Associate*)
John Edward Radcliffe (*from Associate*)
Kenneth George Rendle (*from Graduate*)
Octavio Gaspar DeSouza Ricardo (*from Graduate*)
Robert John Rockcliff (*from Graduate*)
Arthur Phillippo Rich Saunders
George Cleverton Saunders (*from Graduate*)
William Harper Sleigh (*from Graduate*)
Brian Spencer Soan (*from Graduate*)
John Mitchell Stephenson
Reginald Ernest Swinfield (*from Graduate*)
Richard Ivor Beaumont Tavener (*from Graduate*)
Eric Lloyd Thomas
John Walter Charles Whenmouth (*from Graduate*)
Sydney Francis Wilson (*from Graduate*)
Ernest Frederick Winter (*from Graduate*)

Associates

Derek Watson Ashworth (*from Student*)
Samuel Wallace Browne
William Robert Barr Currie
Frederick Algernon Dare
Albert Edward Farmer
Ramsden Fisher (*from Student*)
Norman Hall-Warren
Eric Norman Harris
Douglas Winter Heighway
John Francis McIntyre
Herbert Percival William Martin
John Richard Stanley Overbury
Eric William John Paterson
Ronald George Prior
John Alan Brooke Sharman
Anthony Ian Rawlinson Shaw
Leonard Ernest Smith
John Thompson
John Norman George Thomson
Harry Oscar Thurlow
Peter Alan Wilson
Jack Kenneth Wybrow

Graduates

David William Allen
Willem Benjamin Heyerdahl Blydenstein (*from Student*)
Bernard Arthur Bridges (*from Student*)
Leslie Byram
Lyndon Spencer Compton (*from Student*)
James Peter Dainty
David Charles Findlay (*from Student*)
Peter Ambrose Hearne (*from Student*)
Surindar Bahadur Mathur (*from Student*)
John Edward Moore
Alan George Robertson (*from Student*)
Frank Edwin Roe (*from Student*)
Shamsher Singh Sidhu
David Clay Sugden (*from Student*)
John Roche Thompson

Students

Malcolm Gordon Cherry
Ronald Owen Ellis
Douglas Howard Fears
William Roger Gibbings
Roger Talbot Griffiths
Michael George Hall
Kenneth Leslie Harper
Robert Laidley
Peter Graham Wyndham Lewis
Stuart Vincent Molony
Manjeri Anantanarayana Ramakrishna
Alan George Stokley
Jack Wilson

Companion

Raymond Douglas Lees-Smith

The Journal of the Royal Aeronautical Society

WITH WHICH IS INCORPORATED THE INSTITUTION OF AERONAUTICAL ENGINEERS

VOLUME 58 MAY 1954 NUMBER 521

THE FIRST MITCHELL MEMORIAL LECTURE

The 897th Lecture to be given before the Society was the 14th Main Lecture at a Branch of the Society and also the first Mitchell Memorial Lecture. The Lecture, held under the auspices of the Southampton Branch on 21st January 1954, was by Mr. J. Smith, C.B.E., F.R.Ae.S., Chief Designer, Vickers-Supermarine. Mr. D. B. Smith, O.B.E., B.A., A.F.R.Ae.S., Chairman of the Southampton Branch, presided at the meeting with Sir William Farren, C.B., M.B.E., M.A., F.R.S., Hon.F.I.A.S., F.R.Ae.S., President of the Society.

MR. D. B. SMITH: Tonight is a very great occasion for the Southampton Branch of the Royal Aeronautical Society, for we have at the same time a Main Society Lecture and the first Mitchell Memorial Lecture.

During the past few years the Society has recognised the value of the work done by its branches by allocating each year two or three main lectures to be held at the branches, and this year we in Southampton have received that honour. The Southampton Branch has inaugurated a lecture to be held each year in memory of that great designer, R. J. Mitchell, and we are most fortunate in having his successor, Mr. J. Smith, to deliver the first lecture of the series.

Branches of the Royal Aeronautical Society have been established at places throughout the country where there are centres of aeronautical interest, and Southampton is, I think, unique because we have on our Committee representatives from four different aircraft firms in the district and also from the University.

My pleasant duty tonight is to introduce the President of the Royal Aeronautical Society, Sir William Farren, but before doing so I would like to say a few words of welcome to our many other distinguished guests. First of all, I extend a welcome to our host, the Vice-Chancellor of this University; I also thank him for so generously placing the Lecture Rooms at our disposal throughout the year both for our lectures and committee meetings.

I also welcome the President and several Members of the Council of the Royal Aeronautical Society, and the Secretary and other members of the main Society's staff.

It gives me pleasure to welcome a large number of Chief Designers, Directors and Senior Executives of the firms and the Ministries, and particularly the representatives of the Services—the Royal Air Force and the Royal Navy. We are fortunate in having at least three pilots of the former Schneider Trophy teams, and many other officers of the Services.

I extend a welcome, too, to our neighbouring branches, the newly-formed Boscombe Down Branch and the Isle of Wight Branch. It is very pleasant to have them with us and it shows that the branches are not just individual local units but that they act in a collective way.

AT SOUTHAMPTON—Dr. Gordon Mitchell, Air Marshal Sir John N. Boothman, Sir William Farren, Mr. J. Smith, Air Vice-Marshal S. N. Webster and Air Vice-Marshal R. L. R. Atcherley.

SIR WILLIAM FARREN: A Main Society Lecture at a Branch is something which the Council regard as an event. We in the Council believe that the real strength of our Society is in the Branches and so we do everything we can to stimulate, and to support, their activities.

The membership of the Branches is, characteristically, different from that of the Society in one important respect. The Society is a Society of scientists and engineers, and the qualifications for membership of it are laid down in our Charter and Rules, which are interpreted by the Council, a matter which, I can assure you, gives the Council very often a great deal of thought and takes a great deal of time. We strive always to maintain the high professional status of the various grades. We regard it both as a duty and a privilege and we do try to make sure that the Society, in all its activities, including the interpretation of those qualifications, moves with the times. I do not think we would be doing our duty by aeronautics if we did not. The Branches, however, have members not only in the grades of the Society but—and personally I am very glad this is the case—generally speaking they have at least as many other members who are not in those grades, although we hope one day they will be; whether they ultimately become full members of the Society or not, I would say that we on the Council and the whole of the membership of the Society welcome them, because it is to them that we look for our future strength.

Now I come to the Lecture this evening. After Mr. D. B. Smith's remarks it is unnecessary for me to do more than to say that the lecture is in memory of a friend of many of us; a great man who, to our sorrow, did not live to see the full results of his genius. I think it is fair to say that he would like to be described as the man who made the Spitfire possible; the Lecture will be delivered by another old friend, Joe Smith, and I would say he is the man who made the Spitfire. I cannot imagine a more appropriate man to give this Lecture.

R. J. Mitchell — Aircraft Designer

by

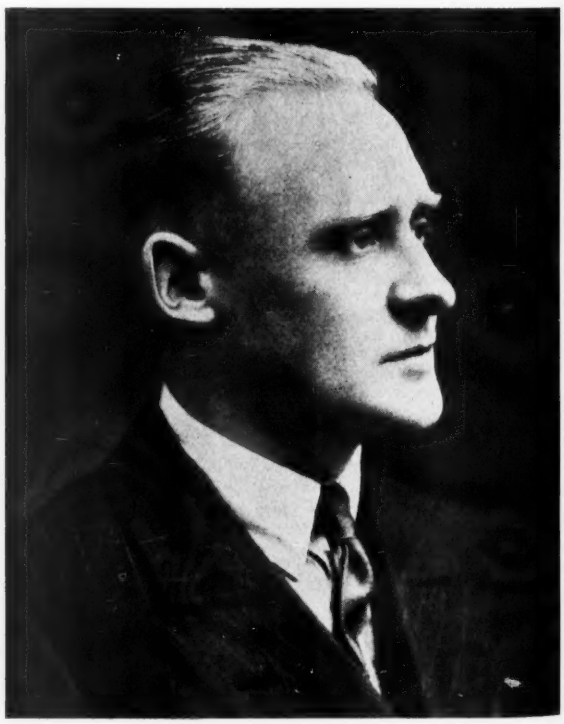
J. SMITH, C.B.E., F.R.Ae.S.

(*Chief Designer, Vickers-Armstrongs Ltd., Supermarine Works*)

I AM very conscious of the privilege and honour of being asked to deliver the first R. J. Mitchell Memorial Lecture. The presence of such a distinguished audience, and the fact that I feel the subject is one to which it is difficult to do full justice, are factors which do not tend to fill me with confidence, and I hope you will forgive any inadequacies. From a mass of recollections arising as the result of fourteen years of contact with Mitchell, I hope to give you my impressions of the life and career of that great designer. I feel rather in the position of a son speaking of his late father, and indeed Mitchell certainly was the technical father of the present Supermarine design team. The affectionate regard however which I held for him during his life was not only made up of respect for his genius, but also for Mitchell the man, with whom I am very proud to have been associated during a most formative period in aircraft development.

Reginald Joseph Mitchell was the eldest of the five children of Herbert Mitchell, a Yorkshire schoolmaster. He was born in the village of Talke, near Stoke-on-Trent, on 20th May 1895. A few months after his birth the family moved to the Longton district of Stoke-on-Trent, and in the ensuing years Herbert Mitchell, the father, was appointed headmaster of a number of schools in North Staffordshire. Eventually, he founded a printing business at Hanley, Stoke-on-Trent.

In the meantime, young Mitchell attended the Queensberry Elementary School at Longton and, in due course, Hanley High School. It was during his school days that he first evinced an interest in what was to be his life's work and is reported to have spent most of his spare time building model aeroplanes. He is said to have carried out his hobby experimenting with various forms of model construction in a schoolroom next to his home, and boyhood friends have described



R. J. Mitchell.

R. J. Mitchell and Captain H. C. Biard (left).

him as being "mad on aeroplanes," but modest, with a likeable, easy manner, saying little, and mechanically minded.

At the age of sixteen he left school and was apprenticed to an engineering firm, Kerr Stuart & Co., engaged on locomotive production. He passed through the shops as an engineering apprentice and finally into the drawing office. He became a skilled mechanic and was keen enough to install a lathe in his bedroom. During this period he attended evening classes in engineering drawing, mechanics and higher mathematics, and later became a part-time teacher in one of the technical colleges. In 1917, at the age of 22, he applied for and obtained a job with Hubert Scott Payne at the Supermarine Aviation Works, Southampton.

Hubert Scott Payne first gave his new employee a job as his own personal assistant, but in the middle of 1918 transferred him as assistant to Mr. Leach, the works manager. When Mr. Hargreaves, chief designer, left the firm in 1919, Mitchell returned to the drawing office as chief designer and, in 1920, at the age of 25, he was also appointed chief engineer.

In 1923, Scott Payne left Supermarine for other spheres in the aeronautical world and the management of the enterprise was taken over by a colleague, Squadron Commander James Bird, who had joined the company in 1919. Bird gave Mitchell his head, and he remained chief engineer and chief designer until 1928. The firm was then taken over by Vickers, and shortly after, Mitchell was appointed director and chief designer, which position he held until his death in 1937. He was elected a Fellow of The Royal Aeronautical Society in 1929, and was an Associate Member of The Institution of Civil Engineers. In 1932 his services to his country were recognised in King George V's Birthday Honours by the award of Commander of the Order of the British Empire.

He was a member of the Air League of the British Empire and a founder member of the Hampshire Aeroplane Club, where he learned to fly, obtaining his pilot's licence in 1934. He was President of the local Unionist Club, and a Freemason of the Lodge of Concord. There is no doubt that, had he lived, honours both public and from the technical societies would have been showered upon him. His most well-known memorial is of course the Spitfire, and all that aircraft meant to this country during the Second World War. I also suggest that what has been accomplished by Supermarine since that date is largely due to his influence on the design team which worked with him, and forms a further memorial to this great designer.

I come now to what is perhaps my most difficult task tonight, to give you a picture of Mitchell's personality. Many of you knew him and I must ask your indulgence for the benefit of those who did not.

He was known as "R.J." to everyone from the lowest to the highest, and the fact that he accepted this nickname is one illustration of his friendliness and absence of false dignity. I shall call him R.J. from now on, because that is how I think of him. He was a well-built man, pleasant faced, of medium height and fair colouring, with a very determined chin. He possessed great charm, an engaging smile which was often in evidence and which transformed his habitual expression of concentration. He was rather shy with strangers, although preserving an outwardly easy manner, and only when one came to know him well did his chief characteristics become evident. Foremost among these characteristics was a clear thinking ability to create, which made him a designer in the truest sense of the word. This creative ability was the driving force of his life, and as we shall see, resulted in a tremendous output of new types of aircraft in an incredibly short span of years. Thinking back, I have realised that no other man in my experience has produced anything like the number of new and practical fundamental ideas that he did during his relatively short span of working life. The wholehearted and continuous application of this genius was an inspiration to all who worked with him.

The next most important characteristic of the man was his notable capacity for leadership. He never shirked full responsibility, and his technical integrity was unquestioned. He won the complete respect and the confidence of his staff, in whom he created a continuous sense of achievement. He placed himself firmly at the helm, and having made decisions, expected and obtained the full co-operation of all concerned. But, in spite of being the unquestioned leader, he was always ready to listen to and consider another point of view, or to modify his ideas to meet any technical criticism which he thought justified. He was always open minded and took a great interest in aeronautical developments. The effect of this attitude on the team of young and keen engineers which he collected around him can well be imagined, especially as he was kindly and approachable and took a lively and sympathetic

interest in the personal problems of those with whom he worked. He was, well, just R.J.

As a person, R.J. was a modest man who hated publicity and shunned public occasions. He was a shrewd assessor of other men and often showed great restraint in dealing with strangers, with whose point of view he disagreed. He had a good sense of humour, or perhaps I should say, of fun, and a capacity to relax completely. If you were away on a business visit with him and the day's work was over, there was always the possibility of some prank developing. He was literally the life and soul of the party on such occasions as the annual drawing office dinner, when with no thought of dignity he became the ringleader in any sort of fun and games, usually aided and abetted by the firm's test pilots. He smoked a pipe, with the aid of a plentiful supply of matches, and was keen on sport. He played tennis, and later, golf, not in order to excel, but as a relaxation, and he was a good loser. He was also fond of a game of snooker and took up sailing as a hobby in later years.

It has been said that he would break off from his social activities to make sketches of aeroplanes on the backs of old envelopes, and that one minute he would be talking, and the next, would be " miles away " in the throes of a problem. Be that as it may, it is certain that his work was never far from his mind and I can remember many occasions when he arrived at the office with the complete solution of a particularly knotty problem which had baffled us all the night before.

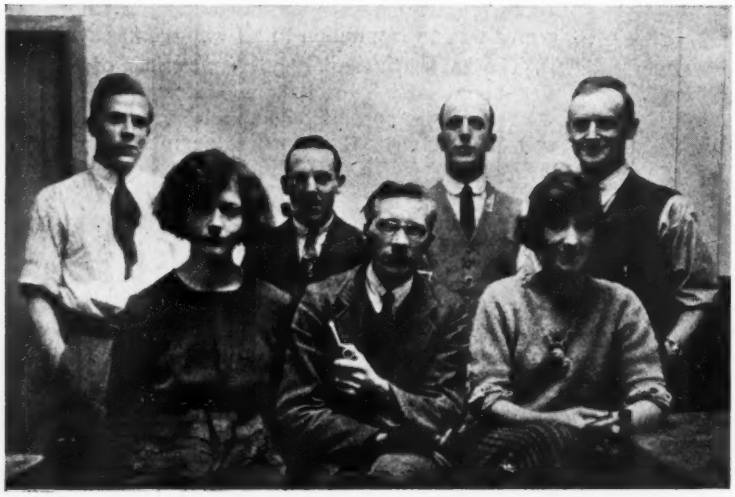
Sir Henry Royce summed up R.J. as, " a man slow to decide and quick to act," and I feel that this is the keynote of the way he worked. A mental picture which always springs to my mind when remembering him, is R.J. leaning over a drawing, chin in hand, thinking hard. A great deal of his working life was spent in this attitude, and the results of this thinking made his reputation. His genius undoubtedly lay in his ability not only to appreciate clearly the ideal solution to a given problem, but also the difficulties and, by careful consideration, to arrive at an efficient compromise.

One result of his habit of deep concentration was that he naturally objected to having his train of thought interrupted. His staff soon learned that life became easier if they avoided such interruption. I have often wondered how I could manage to apply a similar ban to my present staff! If you went into his office and found that you could only see R.J.'s back bending over a drawing, you took a hasty look at the back of his neck. If this was normal, you waited for him to speak, but if it rapidly became red, you beat a hasty retreat! In more serious vein, R.J. was an essentially friendly person, and normally even-tempered, and although he occasionally let rip with us when he was dissatisfied with our work, the storms were of short duration and forgotten by him almost immediately—provided you put the job right!

Another very human aspect of the man which soon became known to his staff, concerned his state of mind during flight trials of a prototype. One did not indulge in silly chatter in light-hearted manner on such occasions. He was always worried for the safety of his pilots and although he witnessed the first flights of so many aircraft, he never grew accustomed to it. During such periods as the testing of Schneider trophy machines he was in a continual state of tension lest a pilot be injured or killed, and felt that he carried a personal responsibility in the matter.

He was an inveterate drawer on drawings, particularly general arrangements. He would modify the lines of an aircraft with the softest pencil he could find, and then re-modify over the top with progressively thicker lines, until one would finally be faced with a new outline of lines about three-sixteenths of an inch thick. But the results were always worth while, and the centre of the line was usually accepted when the thing was re-drawn.

When in the throes of a new design the arrangement of which had been decided, he would spend almost all his time in the drawing office on the various boards. Here he would argue out the details with the draughtsmen concerned, and show a complete grasp of the whole aircraft. It is appreciated that such a method could only be used with the type of aircraft then being built, but it illustrates the practical outlook of the man.



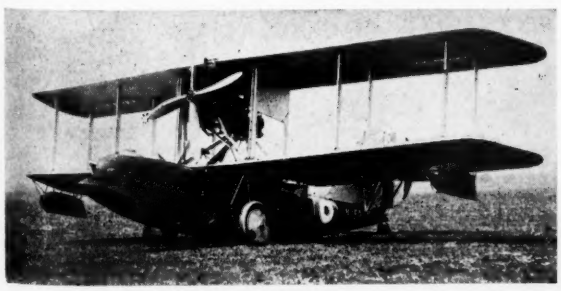
Original staff, 1923:—(*Back row*): A. H. Shervall, apprentice; F. Holroyd, draughtsman; C. V. Kettlewell, draughstman; J. Smith, draughtsman. (*Seated*): Miss Haines, tracer; Mr. Harris; and Miss Attwater, chief tracer.

Construction of the machine having begun, he would spend some time each day examining and assessing the result. If he was not satisfied with the way something had turned out, he would go back to the drawing office and, having discussed the matter with the people concerned, either modify it or leave it as the case might be. And always the practical aspects of the proposed alteration would be borne in mind in relation to the state of the aircraft, and the ability of the works to make the change.

Finally, in this attempt to show you Mitchell the man, I turn to his magnificent courage. In the whole range of human emotions there can be nothing as terrible as the realisation that an incurable disease makes one's death inevitable within a short space of time. To have the courage to face such a tragic fate unflinchingly must be the hope of every man, adding a fervent prayer that it may never happen to him. It did happen to Mitchell, and I can personally vouch for the fact that he behaved in a way which was beyond praise. To talk to him during this period was to see the highest form of courage, and the memory must always remain an inspiration. The facts are tragically simple. In 1933 R.J. was taken ill and underwent an operation which it afterwards became evident was only partially successful. Although he apparently completely recovered, he became increasingly subject to bouts of pain during the next three years, and in March 1937 there was a further operation. It was then found that the disease had progressed to the extent of being incurable, and he was given three months to live. He was told by his surgeon, and he returned home. He proceeded to put his private affairs in order and made wise provision for his wife and son down to the smallest detail. In April he was flown to Vienna to the clinic of Professor Freund, but the disease had progressed too far, and he returned home at the end of May. Three weeks later he died at his home, having been unconscious for several days. He was only forty-two years of age, and his brain was in its prime. A tragic loss to his country, his firm and his friends.

Technical Achievements

The most outstanding feature of R.J.'s technical achievements is the number of aircraft which he designed in the sixteen years between 1920, when he became chief designer, and 1936, when his last illness began. There were twenty-four different types, ranging through bombers, flying boats, amphibians, racing seaplanes, fighters and even light aeroplanes. The amazing diversity of this work is no less remarkable than its amount, and I will now try to give you a brief summary of these aircraft year by year.



The Seal (450 h.p. Napier Lion) amphibian, 1921, developed from the Martlesham and later re-named the Seagull.

The Martlesham amphibian of 1920 with which Mitchell scored his first success.

1920

A Government-sponsored competition for the best commercial amphibian, offering large cash prizes, had been arranged and Supermarine entered a development of the Channel type. It was a 50 ft. span biplane, weighing 5,300 lb., powered by a Rolls-Royce Eagle VIII, with a pusher airscrew. The machine performed so well that although it was placed second, a prize specially increased from £4,000 to £8,000 was awarded. Mitchell had scored his first success.

1921

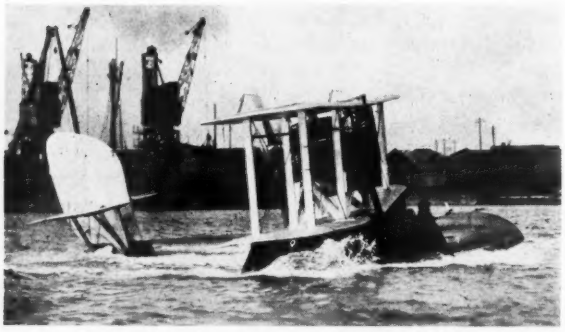
An improved form of the Martlesham amphibian was designed by R.J., which was known as the Seal. It was afterwards re-named the Seagull, and was produced in quantity for the R.A.F. and Australian Air Force, and had the phenomenal operational life of twenty years. Two marks of Seal and three of Seagull were produced during this time, but the basic design remained unaltered. It was a biplane powered with a 450 h.p. Napier Lion engine, with a then unusual tractor propeller. The wings folded backwards and provision was made for a rear gunner. The seaworthiness of the hull was remarkably good. The machine had a span of 48 ft., length 53 ft., fuel for five hours cruising, and a maximum speed of 93 knots and a military load, including fuel, of 1,800 lb.

Also produced in 1921 was the Sea King Mk. II, in which the 160 h.p. Beardmore engine of the original Sea King was replaced by a 300 h.p. Hispano Suisa. This small amphibian with a gross weight of 2,850 lb. and a wing span of 32 ft., was armed with a Lewis gun, and could be looped, rolled and spun.

1922

In this year, for the first time, Supermarine won the Schneider Trophy for Great Britain, at a speed of 145 m.p.h. This sixth contest was held at Naples and Captain Biard piloted the Supermarine Sea Lion II. This machine, which was entered privately by Supermarine, was developed from the Sea Lion I, itself a cleaned-up version of the Sea King, powered with the 450 h.p.

The Sea King II (300 h.p. Hispano Suisa). 1921.



The Sea Lion II (450 h.p. Napier Lion) with which Great Britain won the 1922 Schneider Trophy at 145 m.p.h.

The Swan (two Rolls-Royce Eagle IX), 1924, forerunner of the Southampton.

(*Left*) The Scarab bomber of 1924, a further development of the Sea Eagle.

The Sea Lion III of 1923.

The Sea Eagle (360 h.p. Rolls-Royce Eagle IX), a six-seater commercial amphibian. 1923.

The Sparrow sesquiplane (Blackburn engine) designed for the 1924 Lympne Light Aeroplane Competition.

Napier Lion engine. For the 1922 race the bow of the hull was re-designed, together with the fin and rudder. It was a fairly conventional biplane, with the usual array of interplane struts of wood and fabric construction. Its top speed was 163 m.p.h. The contest was a tense one, in that Italy could have won the Trophy outright, having secured two previous successive victories.

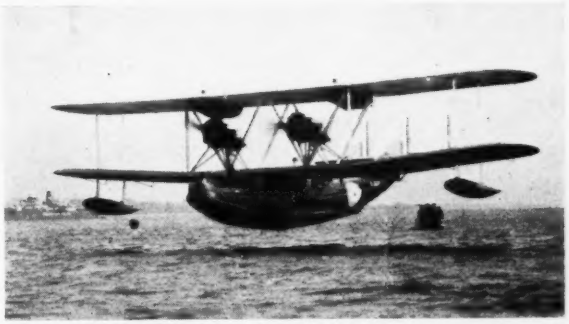
1923

The Schneider Trophy being then an annual event, R.J. made further attempts to clean up the Sea Lion and produced the Mk. III, again without Government backing. The wings were clipped, the engine cowling revised and further hull modifications made. The machine, however, was outclassed by the American Curtiss racer, a floatplane which flew the course at a speed of 177 m.p.h. The Sea Lion's speed was 151 m.p.h., an increase of 6 m.p.h. for the cleaning up process, although it eventually attained a maximum speed of 175 m.p.h. During this year, R.J. designed the Sea Eagle, a commercial amphibian to carry six passengers. This was a single-engined biplane powered by a 360 h.p. Rolls-Royce Eagle IX. It had a range of 230 miles at a cruising speed of 84 m.p.h., and an all-up weight of 6,000 lb. Three of these aircraft were built and operated by the newly-formed Imperial Airways. They remained in service for a number of years without incident and were well liked by the intrepid travellers who used them.

1924

The next aircraft was a further development of the Sea Eagle, and was known as the Scarab. It was an amphibian bomber and twelve were supplied to Spain. It had twin fuel tanks on the upper wing to increase range and carried a bomb load of 1,000 lb. It also carried a camera for reconnaissance duties. These aircraft saw active service in the Moroccan War.

It was in 1924 that R.J. designed the forerunner of that famous flying boat the Southampton. It was a twin-engined commercial amphibian named the Swan, and the first of its type. Two Rolls-Royce Eagle IX engines were fitted and a range of 300 miles at 85 m.p.h. cruising speed was achieved with an all-up weight of



The Southampton I wooden-hulled military flying boat (Two Napier Lions) of 1925.

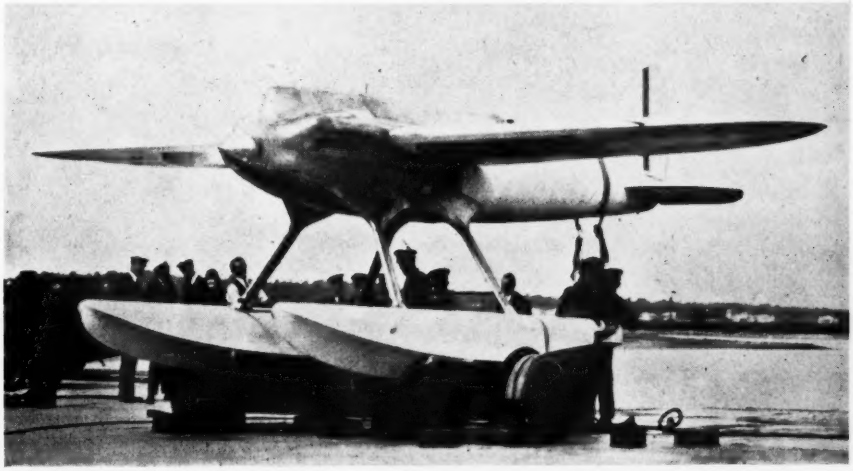
11,900 lb. An ingenious mechanism for undercarriage retraction was incorporated, actuated by an air driven propeller, and the wings folded forwards. It was submitted to Felixstowe for test and such excellent reports followed that an immediate order from the Air Ministry resulted. His Royal Highness the Duke of Windsor, then Prince of Wales, honoured the firm by a visit to inspect the aircraft, and it is of interest to note that *Flight* of 3rd July 1924, in reporting this visit and describing the aircraft, added a footnote which read: "Mr. R. J. Mitchell is now regarded as one of our foremost flying boat designers." This, at the age of 29, was after only four years as chief designer!

For the 1924 Lympne competition, R.J. designed a light aeroplane known as the Sparrow. It was fitted with a Blackburn three-cylinder radial engine and the all-up weight was under 900 lb. A maximum speed of 72 m.p.h. was attained, and a ceiling of 11,000 ft. It was a sesquiplane, the two wings being of different section.

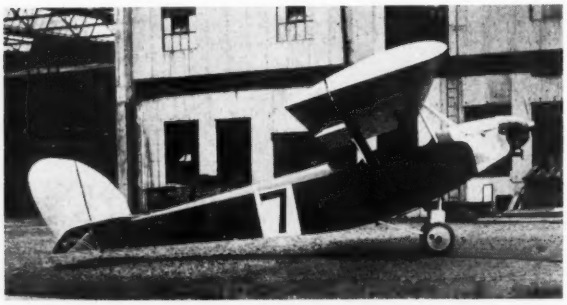
1925

This year was undoubtedly one of the busiest and most successful of his career, for it was during 1925 that both the Southampton Mk. I and the S.4 racer appeared. These two aircraft set the seal on R.J.'s reputation as a designer.

The Southampton I was the military version of the



The S.4 (Napier engine) designed for the 1925 Schneider Trophy Contest.



The Sparrow as a monoplane in 1926 and fitted with a Bristol Cherub engine.

Swan, but was a flying boat instead of an amphibian. With very little alteration it remained in operational service until 1936: at least double the life span allotted to military types of this time. The aircraft was a biplane with a span of 75 ft., a monoplane tail with triple fins and rudder carried directly by the upswept rear end of the hull. It was fitted with two Napier Lion engines driving tractor airscrews, and could climb and turn on one engine. It carried a crew of five, had a maximum speed of 107 m.p.h. and a landing speed of 56 m.p.h. The all-up weight was 14,300 lb, and the range 680 miles. It was of wood and fabric construction, and had a then unusual arrangement of steel interplane struts which were reduced to a minimum in number. It is notable that the first aircraft was produced in 7½ months.

As a result of the 1923 contest R.J. realised the absolute necessity of designing special high-speed racing aircraft and the S.4 racer was the beginning of R.J.'s preoccupation with speed which produced such brilliant successes as the S.5 and the two S.6's, culminating in the Spitfire. Quoting from *Flight* of 24th September 1925—"One may describe the Supermarine Napier S.4 as having been designed in an inspired moment. That the design is bold no one will deny, and we think the greatest credit is due to R. J. Mitchell for his courage in striking out on entirely new lines. It is little short of astonishing that he should have been able to break away from the types with which he has been so intimately connected, and not only abandon the flying boat type in favour of the twin float arrangement, but actually change from the braced biplane to the pure cantilever wing of the S.4!" The subsequent description of the aircraft is dotted with such phrases as "exceptionally clean design," "one of the most daring features," etc.: in other words, the technical Press were very impressed.

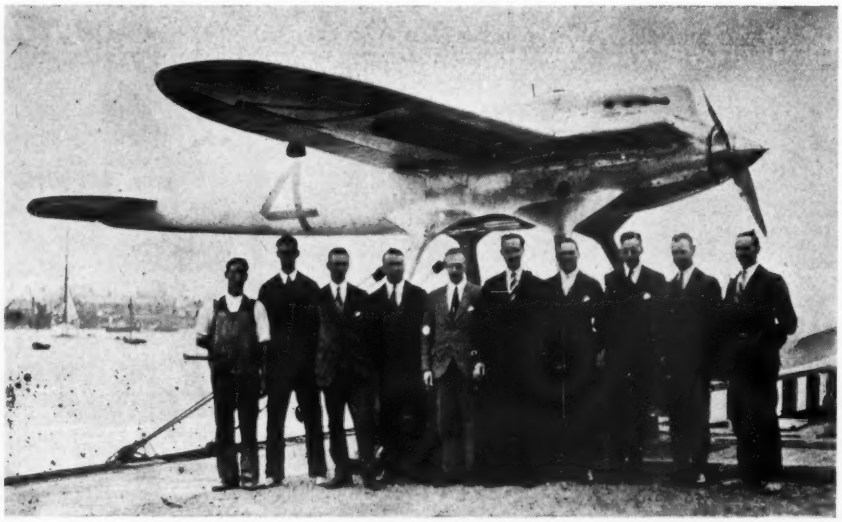
Time will not permit of a detailed description, but some features are worthy of note. Stressed skin construction was largely employed, using plywood as the skin: rigid tubular rods were used for control runs, the ailerons being operated by torque tubes. Trailing edge flaps were fitted to lower the landing speed, a cantilever strut system was employed for the attachment of floats to fuselage and the engine was entirely cowled in. There is little wonder that in 1925 the design was described as "daring." The machine was tested and flown by Captain H. C. Biard and set up a seaplane speed record of 226 m.p.h. It was taken over to America for the race, but unfortunately crashed during pre-race trials.

1926

The Southampton Mk. II was designed this year, or, I should say, the metal hull for this aircraft, other differences being insignificant. The two hulls were completely interchangeable dimensionally, and a saving in weight of 540 lb. was achieved, apart from lack of soakage, which was responsible for another 400 lb. The result was an increase in range of approximately 25 per cent., to 900 miles.

The wooden-hulled Southampton Mk. I had already proved itself by making cruises in formation, one of 10,000 miles over the Irish Sea by four aircraft, and one in the Mediterranean by two aircraft which proceeded as far as Egypt and Cyprus, the first exercise of its kind to be made by the R.A.F.

In this year also, the Sparrow light aeroplane was



The S.4 of 1925. R. J. Mitchell is fifth from the left and the group also includes (fourth and third from the right) Lt. Cdr. James Bird and Captain H. C. Biard.

The S.5 (900 h.p. Napier engine) for the 1927 Schneider Trophy about to be launched. In the left hand corner R. J. Mitchell can be seen in characteristic attitude.

fitted with a Bristol Cherub flat twin engine and converted to a parasol monoplane in order to investigate full scale the merits of various wing sections.

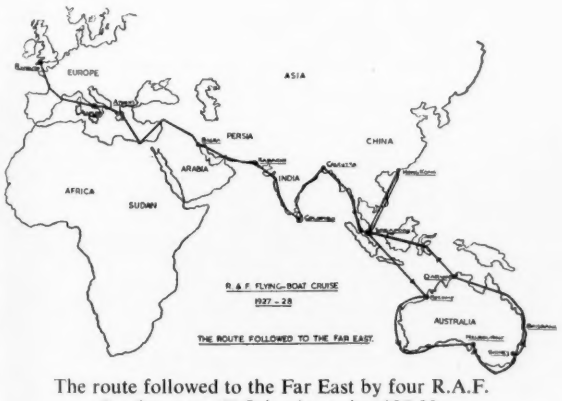
1927

Mitchell's experience gained in designing the S.4 racer now came to full fruition in the S.5. The development of this aircraft from the S.4 is described by R.J. himself in one of the few technical papers he wrote. In this paper he lists the main problems as being

(*a*) Reduction of weight.
(*b*) Reduction of drag.
(*c*) A satisfactory water performance.

Reduction in weight was achieved by reverting to bracing between wings, floats and fuselage instead of the unbraced cantilever structure of the S.4. As a result of this, and every effort being made to reduce the weight of components, a structure weight of 36 per cent. was achieved, compared with 45 per cent. on the S.4.

Reduction of drag resulted from decreased size owing to weight saving, reducing the cross section of the fuselage by as much as 35 per cent., and a reduction in frontal area of the floats of 14 per cent. A comprehensive series of wind tunnel tests was made for the first time on Schneider Trophy racers, the resulting information proving extremely useful in subsequent designs.



The route followed to the Far East by four R.A.F. Southampton II flying boats in 1927-28.

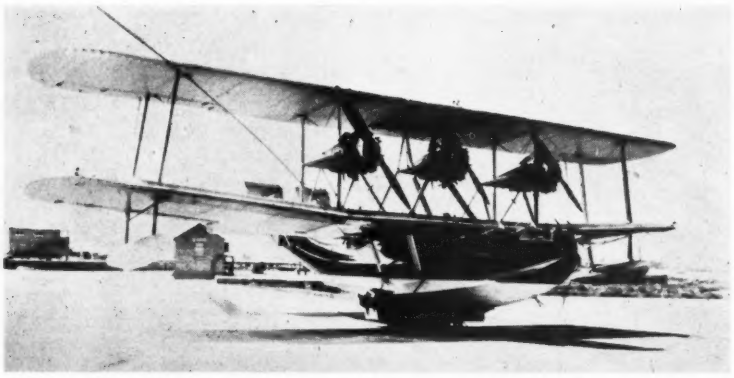
Flat-surfaced wing skin radiators also contributed largely to drag reduction.

Good water performance was evolved by a full series of tank and wind tunnel tests in a determined effort to combine satisfactory handling on take-off with low drag floats. No less than ten different forms of float were tried until a satisfactory compromise was arrived at. The aircraft was powered with a Napier engine of 900 h.p.

One comment in Mitchell's paper is worth quoting as giving an insight into the way he worked. He says, "The foregoing notes serve to indicate what was done in the case of the S.5. Whilst these efforts resulted in a success which was gratifying, their real value lies in the experience gained."

The design and construction of the S.5 had full Government backing, and three aircraft were built. Later at Venice, Flight Lieut. Webster won the Trophy at a speed of 281 m.p.h., and another S.5 took second

A Southampton III, 1928.



The Nanook (three Armstrong Siddeley Jaguars) developed from the Southampton in 1928. It was later re-named the Solent.

place. The following year a British speed record of 319·6 m.p.h. was set up.

It was also during 1927 that the Southampton made the historic Far East flight under Group Captain H. N. Cave-Brown-Cave, which established this aircraft as being an outstanding success. Four machines flew a total of 23,000 miles in formation by way of Syria to Basra, down the Persian Gulf to Karachi, on to Calcutta, and through the East Indies to Australia. They then circled the entire continent of Australia, returning to Singapore, followed by a trip to Hong Kong, and finally took up station at Singapore. Although such an operation would be fairly commonplace today, it aroused outstanding world interest at the time, adding greatly to the prestige of the British Aircraft Industry, and to R.J.'s reputation.

1928

During this year a development of the Southampton was built, known as the Nanook, and was powered by three Armstrong Siddeley Jaguar engines. With a 75 ft. wing span it had an all-up weight of 16,300 lb., a range of nearly 1,000 miles, and a top speed of 110 m.p.h. It was later re-named the Solent, being then converted to a private air yacht for the Hon. A. E. Guinness, in which form it gave many years of excellent service.

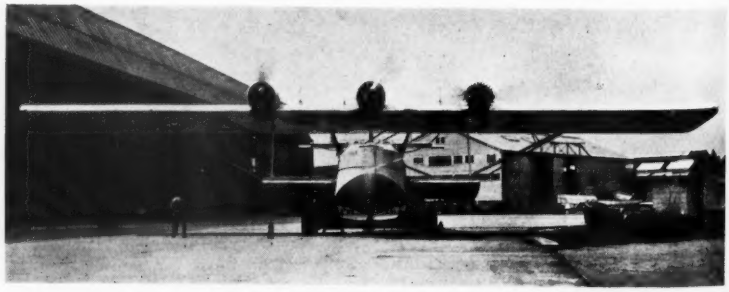
(*Above*) The Seamew (two Armstrong Siddeley Lynx) amphibian. 1929.

1929

Mitchell's third racer, the S.6, was built in 1929. Rolls-Royce had produced a new engine of 1,900 h.p. as against the 900 h.p. of the 1927 Napier Lion, and R.J. then began that close co-operation between Supermarine and Rolls-Royce which has lasted till the present day. There were only small points of difference between the S.5 and S.6, apart from the engine, and the resulting alteration to cowling lines. The forward float struts were re-positioned to support the engine, the wings as well as the fuselage were of metal, and re-designed double skin wing surface radiators of light alloy replaced the copper version used on the S.5. The aircraft fin was used as the oil tank and additional cooling was provided by double skin internally baffled radiators attached to the fuselage sides. Flying Officer Waghorn won the Trophy at an average speed of 328·6 m.p.h., and, later, Squadron Leader Orlebar established a world's speed record of 358 m.p.h.

Also in this year a biplane amphibian known as the Seamew was produced. It was a three-seater reconnaissance machine, fitted with two Armstrong Siddeley Lynx engines. It had a span of 37 ft., an all-up weight of 6,500 lb. and a maximum speed of 100 m.p.h. In appearance, it was rather like a miniature Southampton.

The Hon. A. E. Guinness, after flying the Solent, placed an order with Supermarine for a further air yacht to his own specification, which was designed and built in 1929. The aircraft was a departure from R.J.'s previous



(*Right*) The Air Yacht (three Armstrong Siddeley Jaguars) specially built for the Hon. A. E. Guinness in 1929.

boats, being a three-engined parasol monoplane in which sponsons were used instead of wing tip floats. Three Armstrong Siddeley geared Jaguar engines were mounted in the leading edge of the wing. The interior of the hull was luxuriously fitted and appointed for cruising with sleeping accommodation.

1930

During this year the Southampton Mk. X was designed and built. This was a three-engined boat with a sesquiplane superstructure powered by Armstrong Siddeley Jaguars. The hull was flat-sided to facilitate production and its under water portion was plated with stainless steel.

In 1930 a contract was placed with Supermarine for a big flying boat similar to the Dornier D.O.X. in size. Much of the design work was completed when the Government of the day cancelled the contract on the ground of economy. It was to have been a six-engined flying boat weighing 75,000 lb. powered with Rolls-Royce H engines, and capable of carrying forty passengers.

1931

The economy urge was still in force when participation in the 1931 Schneider Trophy race was considered and, to the dismay of Supermarine, the Government decided not to finance the necessary aircraft. At a very late stage however Lady Houston presented £100,000

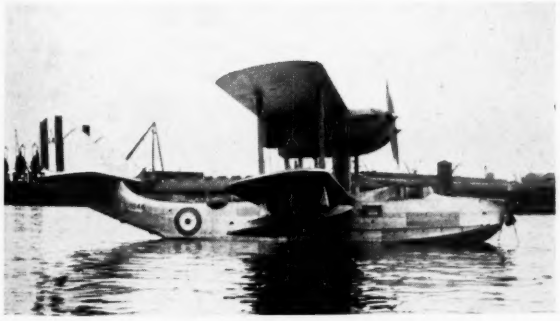
The Southampton X (three Armstrong Siddeley Jaguars) of 1930.

to the Government to enable Britain to enter. There was no time to design a new racer, but Rolls-Royce had produced the R engine, developing 2,330 h.p. Modifications to incorporate the new engine, chiefly increased cooling and larger floats carrying more fuel, were hurriedly made to the original S.6's. These were known as S.6A. Two new aircraft, known as S.6B, were also built.

These aircraft won the 1931 contest unopposed at an average speed of 340 m.p.h., and afterwards put up a world's speed record of 407 m.p.h. The pilots were Flight Lieut. Boothman, now Air Marshal Sir John Boothman, and Flight Lieut. Stainforth, and the 407·5 m.p.h. still stood as a British record fourteen years later.

1932

A replacement for the Southampton was now designed by R.J., known as the Southampton Mk. IV, and later as the Scapa. It was a twin-engined bi-plane flying boat fitted with Rolls-Royce Kestrel III M.S. engines of 525 h.p., accommodated in nacelles close under the top wing to increase the water clearance of the airscrews. The hull was larger than that of the Southampton, and more straight-sided, while the fabric



(*Above*) The Scapa (Two Rolls-Royce Kestrel IIIs) flying boat replacement for the Southampton in 1932.

(*Left*): The S.6 (1,900 h.p. Rolls-Royce) 1929 Schneider Trophy machine on a special towing lighter.

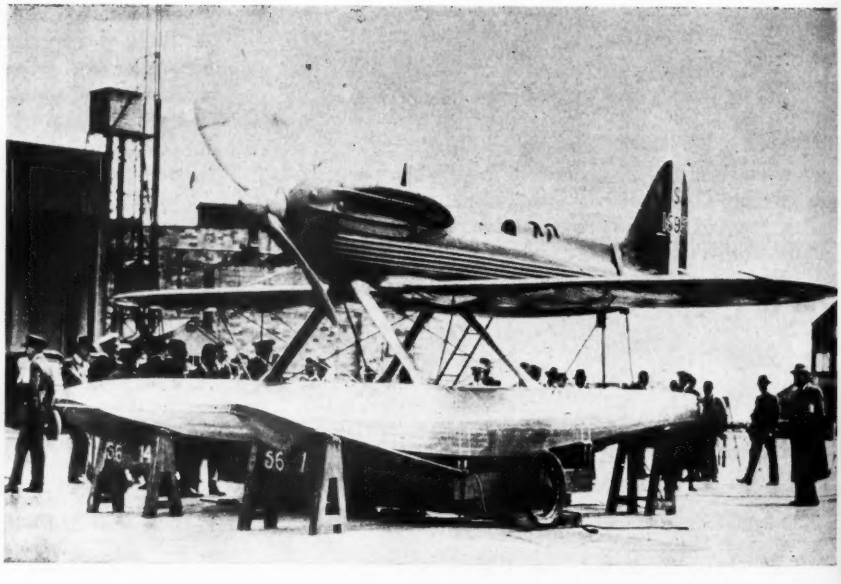
covered wings, tail unit and rudder and fins were for the first time constructed of light alloy. Engine cooling was achieved by honeycomb radiators fitted aft of the monocoque nacelles, and controlled by hand-operated shutters. The wing span was 75 ft. and the all-up weight of the aircraft 16,000 lb. The maximum speed was 142 m.p.h., with a landing speed of 64 m.p.h., and a range of 1,100 miles. The cockpit was enclosed and arranged for dual control. Like its famous predecessor, it could be flown on one engine at full load, and was very seaworthy. It was supplied in numbers to the R.A.F.

It was also in 1932 that R.J. began once more to think in terms of high-speed aircraft based on an official fighter specification, F.7/30. This specification tied down the designer severely, although it undoubtedly served a useful purpose in calling for a single seater, single-engined monoplane with a performance far in excess of the best biplanes then in service with the R.A.F. The Supermarine version was a 46 ft. span cranked low wing monoplane powered by a steam-cooled Rolls-Royce Goshawk engine of 600 h.p. An open cockpit was used.

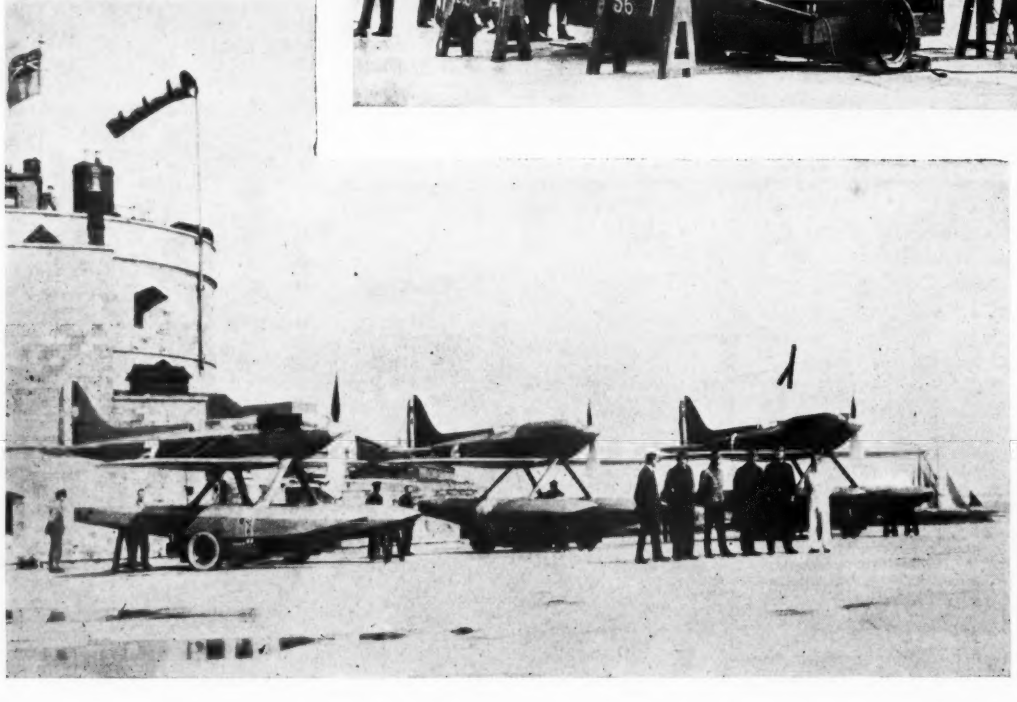
1933

This was the year of the birth of another service type which is still remembered with affection by the Navy, and which remained operational for Air Sea Rescue work for twelve years. I refer to the Walrus, commonly known as the ' Shagbat,' which was designed as a private venture by Supermarine and was originally known as the Seagull Mk. V. It was a single-engined amphibian

(*Right*) The S.6B (2,330 h.p. Rolls-Royce R engine), 1931.



(*Below*) The 1931 Schneider Trophy entries at Calshot, an S.6A and two S.6Bs, all with Rolls-Royce R engines. The Trophy was won by Flight Lieut. J. N. Boothman in an S.6B at 340 m.p.h.

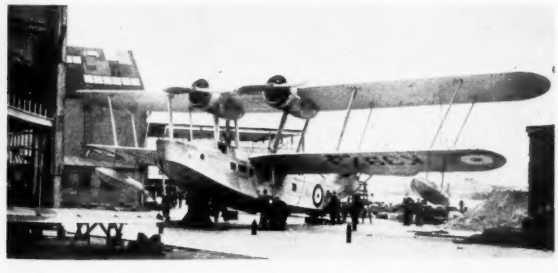


powered with a Bristol Pegasus engine driving a pusher airscrew. Outstandingly seaworthy, it was designed for use on capital ships and carried machine guns fore and aft. The hull was of normal duralumin construction, but the wings had stainless steel spars and wooden ribs, and were fabric covered. The cabin was totally enclosed and immediately behind it was a fully equipped navigator's compartment; the wing span was 46 ft. with a folded width of 17 ft. 6 in. The all-up weight of the aircraft was 7,200 lb., and it had a maximum speed of 135 m.p.h.

1934

The last of Supermarine's biplane flying boats was designed in this year, namely, the Stranraer. This aircraft was powered by two Bristol Pegasus X engines of 810 h.p., a larger hull was fitted, the wing span increased to 85 ft. and the all-up weight to 19,000 lb. The range was still 1,100 miles but the rate of climb was more than twice that of the Scapa, and the top speed was 165 m.p.h. Designed for all-round military use, including torpedo carrying, it was outstandingly successful in its trials, exceeding all the specification requirements by large margins. It was ordered by the R.A.F. and it is interesting to note that it was also built in Canada, where the aircraft remained in use until very recently.

Although the estimated performance of the F7/30 was achieved, R.J. felt that a considerable improvement could be made. The experience he had gained was of the greatest value, and work was begun immediately on



The Stranraer (two Bristol Pegasus Xs) of 1934—the last of the Supermarine biplane flying boats.

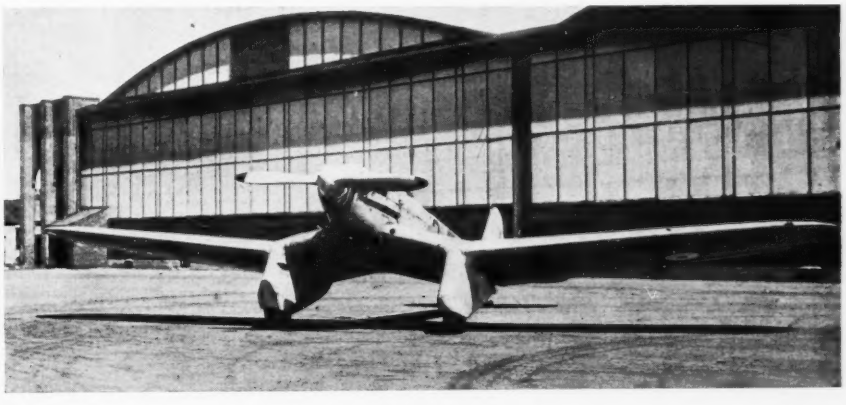
The Walrus (Bristol Pegasus) of 1933 known to many R.A.F. and Navy personnel as The " Shagbat."

a further project, a design which ultimately became the Spitfire. The steam-cooled Goshawk engine was retained, but the machine took the form of a low wing monoplane with greatly reduced wing area and with the pilot's cockpit totally enclosed. This design was submitted to the Air Ministry and accepted, with certain modifications. A period of intensive work on this design followed, influenced by three major considerations, the necessity for retracting the undercarriage, the advent of the Rolls-Royce Merlin engine of 1,000 h.p., known originally as the P.V.12, and the Air Ministry requirement for the machine to be fitted with eight machine guns. The advantages to be gained by the first two considerations were obvious, and the wisdom of the third was apparent even in those days. These were incorporated in the final design, and an Air Ministry specification, F.37/34, was issued to cover it.

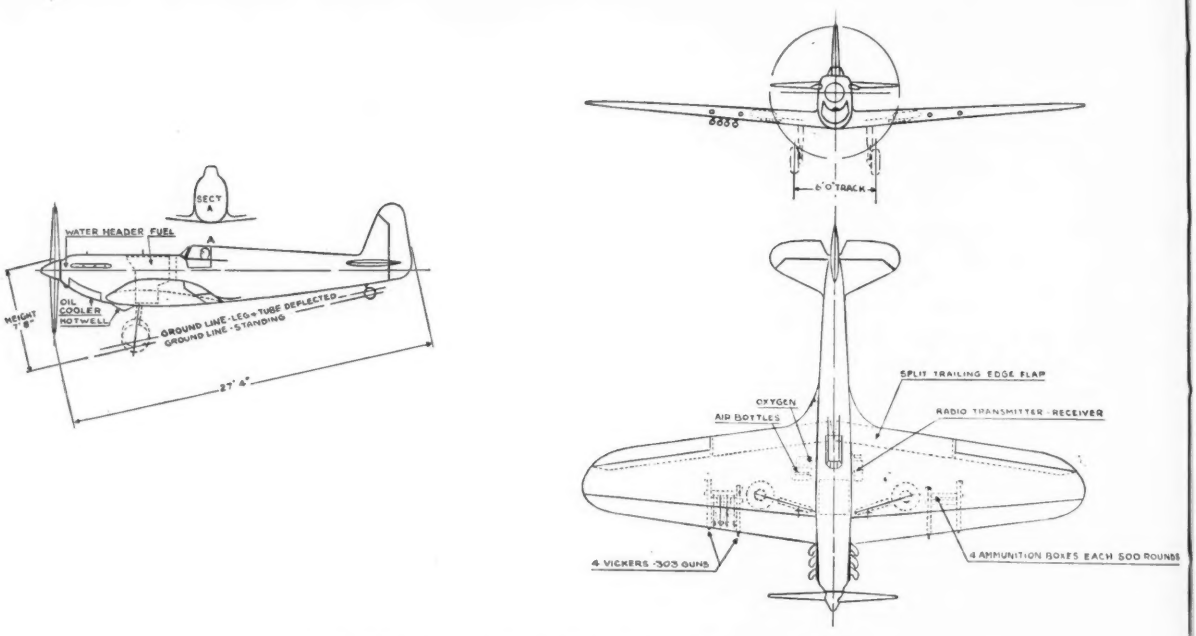
1935

Early in 1935 Supermarine received a contract for a prototype Spitfire, and work began. R.J. gave the job his detailed attention, and during the year the various components were finalised.

A contract was also placed by the Air Ministry for a four-engined bomber to specification B.12/36. This was the last aircraft R.J. designed, and there seems little doubt that it would have been highly successful. It



(*Right*) The F7/30 single-seat fighter (600 h.p. Rolls-Royce Goshawk) designed in 1932 to Air Ministry specification.

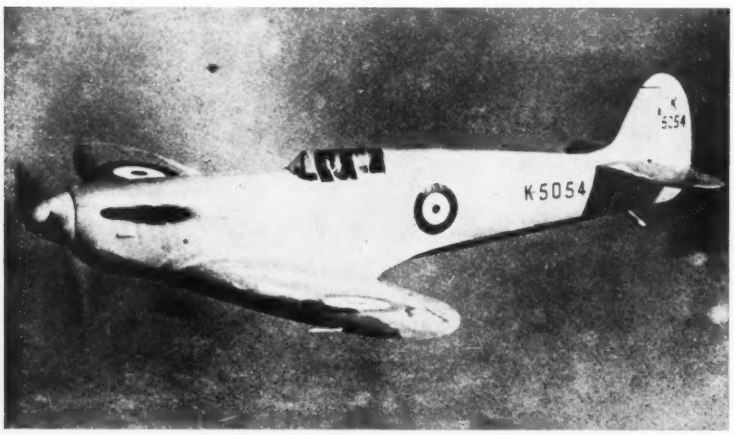


F7/30 Development, 1934, which ultimately became the Spitfire.

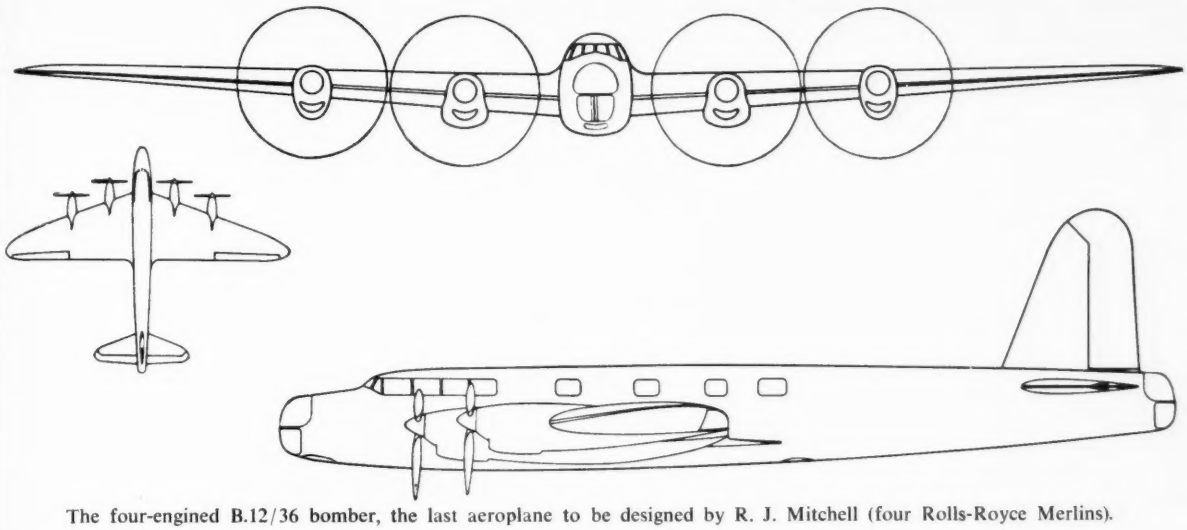
incorporated a number of novel features, some of which have since become commonplace. The aircraft was a 93 ft. span, mid-wing monoplane of swept-back plan form, with a choice of engines, *i.e.* the Rolls-Royce Merlin, Bristol Hercules or Napier Dagger. It had an estimated all-up weight of 55,000 lb., maximum speed of 370 m.p.h. and a range of 3,000 miles carrying 8,000 lb. of bombs. The wing was of the single spar type, using hollow tapered extrusions for the booms, and was metal covered. The main fuel supply was carried in leading edge tanks which were part of the structure. The use of the torsion box single spar principle allowed the carriage of bombs in the wing aft of the main spar in a single layer. The undercarriage and tail wheel had twin wheels. Power-operated gun turrets were fitted in the extreme fore and aft ends of the fuselage, with a retractable turret amidships. A variety of bomb loads was catered for, including twenty-nine 250 lb. bombs and seven 2,000 lb. bombs. A large proportion of the design work and approximately half the construction was completed when the fuselages were destroyed by enemy bombing and the work was then abandoned in favour of increased Spitfire production.

1936

This was the last full year of R.J.'s life, and his health was failing, with consequent absences from work. He carried on when possible with the work on the bomber, and on 5th March he witnessed the first test flight of the prototype Spitfire K.5054, made by Mutt



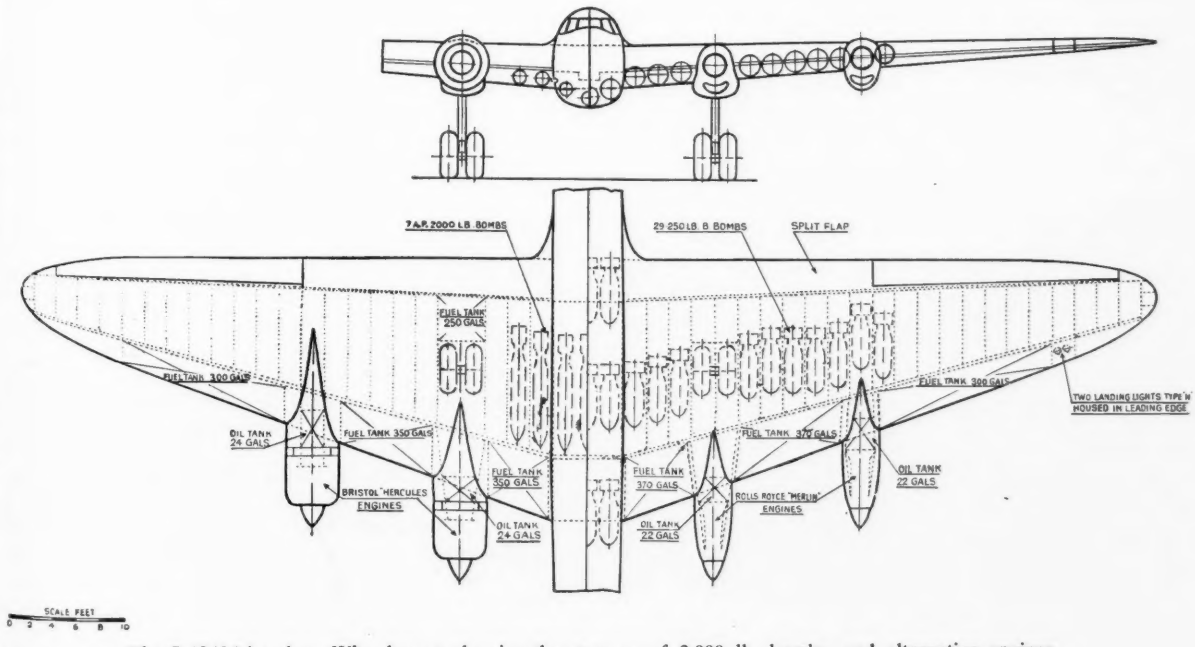
The prototype Spitfire (Rolls-Royce Merlin) which first flew in March 1936.



The four-engined B.12/36 bomber, the last aeroplane to be designed by R. J. Mitchell (four Rolls-Royce Merlins).

Summers at Eastleigh Airport. These tests, and those which followed, soon established the fact that he had produced the fastest and most efficient fighter in the world. In June 1936 the Air Ministry awarded Supermarine a contract for a large number of these aircraft, and Mitchell was satisfied that he had done his part in providing the country with a means of defending its freedom. This thought sustained him until the end.

During the period of seventeen years which has elapsed since his death, enormous changes have taken place in aircraft design, stimulated to a great extent by the Second World War. It is of great interest to wonder what R.J.'s reactions would have been to the progress which has been made so far. What would have been his conception of an aircraft powered by a gas turbine? Would he have solved the aerodynamic problems which this revolutionary change made necessary, sooner than his contemporaries? Would his approach have followed similar lines to those which have been adopted, namely, swept-back wings and tail units, delta configurations and no tail units, exceedingly thin wings with all their attendant structural difficulties, power-operated control surfaces and the numerous complications which the gas turbine with its installational complications and high


7 & P. 2000 LB. BOMBS
29 250 LB. B. BOMBS
SPLIT FLAP
FUEL TANK 250 GALS.
FUEL TANK 300 GALS.
FUEL TANK 300 GALS.
TWO LANDING LIGHTS TYPE 'N' HOUSED IN LEADING EDGE
OIL TANK 24 GALS.
FUEL TANK 350 GALS.
FUEL TANK 370 GALS.
OIL TANK 22 GALS.
BRISTOL 'HERCULES' ENGINES
FUEL TANK 350 GALS.
OIL TANK 24 GALS.
FUEL TANK 370 GALS.
OIL TANK 22 GALS.
ROLLS ROYCE 'MERLIN' ENGINES
SCALE FEET
0 2 4 6 8 10

The B.12/36 bomber. Wing layout, showing the stowage of 2,000 lb. bombs, and alternative engines.

fuel consumption have made necessary? What would have been his reactions to the complicated and bulky equipment which has now to be fitted, and what would he have have thought of present-day armament requirements? Further, it is interesting to speculate and wonder whether he ever foresaw the possibility of having to pressurise aircraft, and to provide temperature control and physical aids for pilots to enable them to withstand the arduous conditions imposed by modern aircraft. Would he have looked into the future and wondered whether, having conquered the sonic barrier, other unknown problems would remain to be tackled, such as, for instance, what might be called the thermal barrier?

R.J. was, above everything else, a practical engineer, and possessed an abundance of plain straightforward common sense. I believe myself, and this is purely my own opinion, that he would have foreseen in good time all these problems rearing their ugly heads, and would have thought out solutions to them. But, with all his skill and dogged determination, I think it possible that he may have greatly exceeded our rate of progress.

Many aspects of Mitchell's character have been portrayed, either through the media of Press, radio or films: tonight I have tried to give you a picture of the man, as we, his colleagues, saw him, and if I have in any way succeeded in doing this, then I feel that I have paved the way for further Mitchell Memorial Lectures.

I should like to close this talk by saying that I should be more than happy if I have succeeded in awakening the ambition of even a few of the younger members of my audience to try to emulate R. J. Mitchell. There is no doubt that in order to carry on the great work which he started, more and more qualified technicians with his kind of drive and genius will be required, not only in the design field, but also in those of mathematics, physics, engineering and electronics. I am quite certain that no better memorial to the memory of R. J. Mitchell could exist than the plentiful and continued flow of technically qualified men into the Industry which he served so well.

FURTHER TRIBUTES

SIR WILLIAM FARREN: After a paper of this kind, which I think must have moved all of you by its simplicity and sincerity, there is nothing I think that would be appropriate in the way of a Discussion, but I know there are many friends of the late Mr. Mitchell in the audience and I think they would like two of the men who, I suppose, of all of us knew Mitchel and his work best, to say a word or two in memory of him. First I will ask Air Marshal Sir John Boothman, who flew one of Mitchell's aeroplanes to victory.

AIR MARSHAL SIR JOHN N. BOOTHMAN, K.B.E.: Joe Smith's talk was a beautiful tribute to a man we all liked. One thing which struck me about R.J. was that he thrived on adversity: when things went wrong he loved it, when things went right he was rather nervous, and I always remember he had a peculiar habit of putting his chin down and then that wonderful grin spread over his face when he thought of the answer. In 1931 two reconstructed machines came along; we had engine failure after engine failure, due to the new fuel taking away the compound which sealed the tanks and also from the new Superflexit pipes, and R.J. sat down in his office, saying nothing, and suddenly a broad grin spread over his face and he said: "You just ——— well fly them until that stuff all comes off!"

R.J. once said to me if he had his life again he would not have been an aeroplane designer but a surgeon—this was a long time before he was sick—and I remember about 1932 or 1933 an occasion when, having been out shooting with him in the early morning, there was a most gory procedure going on in the kitchen when he was explaining the inside of a rabbit to his son.

R.J. was a very great man and I am very proud to have been allowed to say my little bit this evening.

AIR MARSHAL SIR RALPH SORLEY, K.C.B., O.B.E., D.S.C., D.F.C., *Fellow*: I cannot reminisce about Mitchell as a man very much because it has all been said. He had great personality, and as a pilot I always enjoyed myself enormously with him because he could talk pilots' language, could understand the pilot's feelings and expressions, and generally speaking you could get on with him. He could understand what you thought about it and he would tell you what it all meant.

I was privileged to have a collaboration with him which proved to be of great importance and of that collaboration, in putting the guns in the Spitfire, I think the best I can do is to tell you, mainly for the benefit of the younger generation, why it was eight. Now the number was not new: I had indeed seen aeroplanes in the First World War covered in guns along the top plane of a biplane, sometimes numbering no less than twelve. I was told by one pilot who had put them there that it helped to keep the aeroplane back, and as he wanted to get behind the enemy he thought that was a very good way of doing it!

The point came when Mitchell saw, I think more plainly than anybody else at that time—and mark you this was in 1934, a long time before the War started—that German effort, and particularly German aircraft design effort, was something that we were going to feel and suffer from. He only made a visit to Germany once and he came back terribly serious-minded on what lay before us, so that when it came to collaboration, in Mitchell I found we both had exactly common ideas on what was in front of us.

Do not forget that the Spitfire, as it was evolved, was full of unorthodox features so far as Service pilots were concerned: it had a retractable undercarriage, enclosed cockpit, and being a monoplane this alone was unusual

for fighter pilots. To a fighter aircraft weighing, at the time, something like 6,000 or 7,000 lb., opposition would come easily enough from pilots who were used to flying and manoeuvring fighter types of much less weight and lighter wing loading having extreme manoeuvrability, so that in front of R.J. lay the problem of putting it over to the Service when it was finished. He was undaunted by such things as that.

We decided that the problem was first to catch the bomber and then kill him in a matter of two seconds, because we thought that there would be no time for second attacks; there would be, indeed, little time to catch the bomber at all, and added to that I felt that if you could hold your sights on a target for two seconds at that sort of immense speed, as it seemed to us at the time, the average pilot was going to be doing just about all he could do. Having got that right it then had to be worked backwards—what sort of lethal density could you deliver in that time?—and there were various choices to consider. There was the ·303, the ·5 and in France, in a very secret form, was the first attempt at the Zomm Hispano gun, and we had to weigh matters very carefully as to which of those three would be the best. The Hispano was a little too previous, because it needed such a very rigid mounting that Hispanos themselves had chosen to use the engine as the base-plate for the gun so as to make it steady enough in firing. This limited the gun to a special engine and one gun. Gradually we worked backwards, and by going for a new ·303 gun altogether, the Browning, and getting the rate of firing accelerated to its maximum, and by not firing any guns through propellers which interrupted the rate of firing, the whole concept became clear. There were the wings of the monoplane and if the guns could go into the wings and so keep the frontal area of the fuselage to the minimum; while doing away with the archaic business of loading guns in the air and clearing stoppages, and all those things that the Service had long got used to, one could make a simple, although unorthodox, armament which we believed would fit the conditions.

Just to reminisce on how that turned out, I remember one day when the aeroplane was at Martlesham, going through its trials, I found it stuck up on the armament butts with an infuriated armament officer tearing the inside out of the wings, and I leaned over him and said: "What do you think of this?" He replied: "Well, I only wish I could find the silly so-and-so who, not content with putting eight guns in, with all this new method of mounting, he chose to have new guns as well and we do not know anything about them!" Although the Spitfire wing was so very thin, Mitchell—and it was Joe Smith who did the job, I think—managed to install all those wretched pieces of ironmongery, and I think he was pleased to do it. Mitchell felt that it was a simple concept and it was unorthodox, and I think that fell into his whole make-up, as you have heard so well described by Joe Smith. And it should not be forgotten that this wing enabled the Spitfire to achieve a Mach number of 0·92, away ahead of any other type.

If any epitaph is ever needed for what I hope will be an immortal aeroplane, I can only suggest the words that the pilots used when describing their contests after the heat of battle; it was so frequently heard, both in the Battle of Britain and all over the world—"Oh, I just gave them a squirt."

SIR WILLIAM FARREN: I did not expect that I should be reminded of my own knowledge of R.J., but two things that Sir Ralph Sorley said have reminded me of something about the Spitfire that even Joe Smith has not mentioned. I always felt, from my own experience of it, that it was the nicest aeroplane to fly that anybody had ever produced; it was a beautiful piece of aerodynamics. Those of you who design aircraft should never forget that you can make aeroplanes which are really nice as well as good aerodynamically, which sometimes people think is almost a contradiction in terms.

I always thought the arrangement of the dashboard on the Spitfire and all the things you had to handle was remarkable for the fact that it was almost a complete contradiction of the accepted tradition, either before or after. It was easily the best that was ever devised, so I hope those who puzzle their heads about standardised cockpits will sometimes think of that.

One more personal memory of R.J. which has a bearing on what Joe Smith said towards the end of his lecture, about R.J.'s thought for the young men who would do the work in the future. I think the only time he and I ever really got down to something was when he spent a considerable time one afternoon discussing with me the problem of the education of his son. I was busy, and had been for a number of years, in that field and only afterwards did I realise what a privilege it was to be asked about such a thing by such a man.

I shall now ask one of the many former Presidents of this Society, most of whom, I am sure, knew R.J., to propose a vote of thanks to our lecturer. We welcome back Major Bulman from a well-deserved holiday in the East.

MAJOR G. P. BULMAN: In this gathering of friends and disciples of R.J. all of us who were privileged to know, and perhaps to work with him, must have flooding in on us a host of memories. I first had dealings with R.J. in 1928. I had but recently taken over the direction of engine development in the Air Ministry and he came to me harassed in soul as to the engine he should choose for the Schneider Trophy in 1929. Should he stick to his previous stable which had brought him such success, the Napier Lion, or should he, perhaps dangerously, plunge too far and select the then untried Rolls-Royce engine? I saw him in great travail, which I shared because we had a great combined responsibility: it would be for me to help carry his choice into execution by getting the cash from my masters. He asked me frankly what I thought of those chaps in Derby and it did not require an awful lot of thought because I knew them very well: I said, "I have a hunch that men like Rowledge, Elliott, Hives and Lovesey will do it if we give them the chance, and he said, "Right, that has decided it," and so the Rolls-Royce engine was selected, with the success you know.

I remember, too, in 1931, the night before the contest we walked down to the Hard and they were running up

the engines, and some idiot came capering up to us and said, "The cooling won't work; there's a cold patch in the wing." Poor R.J., I thought he was going to leap into the sea, but he soon recovered. And then I remember he and I went to the Folies Bergère together one night—to discuss streamlining, not of aircraft!—and in the interval, as not unusual, we betook ourselves to the bar for a little refreshment and there, as not unusual, we were rather bothered with people walking up and down and trying to attract our attention. R.J., bored with this, fiddled about and took some francs out of his pocket in order to get rid of one such individual. We then went back and enjoyed the rest of the show and when it was over went round to collect our coats; R.J. had just bought a new one. He felt in his pocket and found he had no ticket and realised with horror that he had given his cloakroom ticket away with his francs. We could see his coat behind the counter but the cloakroom man, with typical Gallic logic and determination, said, "No ticket, no coat." Something had to be done, so I engaged the attention of this cloakroom man with my horrible French and worked up a terrific row, getting him towards the end of the counter, and Mitch watched his opportunity, leaped over the bar, grabbed his coat, and we both dashed out and tore down the road like pickpockets.

My task is to move a vote of thanks to the lecturer. He has painted with a loving pencil a most perfect portrait of a great man. Nobody, I think, could have done it better. I was particularly interested because in a few weeks' time I am to have the privilege of doing likewise for Frank Barnwell in Bristol. Mr. Smith brought out the greatness of R.J. and indicated that, like all great men, he did not suffer fools gladly; on the other hand he was always so ready to discuss and argue. He was not one who bit the hands of those who tried to help or who imputed false motives to those who perhaps disagreed in the endeavour to get something better. He was just out for the best and to accept what help was offered; a very simple, a very modest, and a most lovable man. I can think of nobody better than Joe Smith to pay this great tribute to R.J. It was he who picked up the torch from R.J.'s hand: he said tonight he felt rather like a son following in his father's footsteps, and so, indeed, Joe has.

THE PRESIDENT: You have shown by your applause how much you agree with Major Bulman. I also feel that nobody could have done the job better than Joe, and I am sure we are grateful to him. This lecture has been a memorable occasion for me—surely one of the most memorable of these Branch lectures, the second of the named lectures in honour of great men. The first was the Rex Pierson lecture at Weybridge, tonight is the first Mitchell Lecture, and, as Major Bulman has reminded us, in a few weeks time there will be the first Barnwell Lecture at Bristol. I think we have chosen well the men we are honouring and we have chosen our lecturers well also.

Stability and Control in Aircraft Design

by

J. C. WIMPENNY, A.F.R.Ae.S.

(The de Havilland Aircraft Co. Ltd.)

The 890th Lecture to be given before the Royal Aeronautical Society was held at the Institution of Mechanical Engineers, Storey's Gate, London, S.W.1, on 12th November 1953, and was presided over by Sir William Farren, C.B., M.B.E., M.A., F.R.S., F.R.Ae.S., President of the Society. Introducing the lecturer, Sir William said that Mr. Wimpenny had been with the de Havilland Company throughout his career in aviation and had been responsible for some years for the design and development of stability and control of the de Havilland aircraft, including the Comet, so that clearly he spoke with a wide knowledge of his subject.

1. Introduction

For many centuries man has longed to fly, but despite many attempts he only succeeded 50 years ago. Why did he fail?—he did what appeared to be the sensible thing and modelled his attempts on the birds. He failed for two reasons:

(*i*) His aircraft were unstable and could not be controlled

(*ii*) He had insufficient power,

and of these two problems the second was relatively unimportant while the first remained unsolved. Thus the problems were solved in that order, first came the glider with a reasonable measure of stability and controllability, and then the powered aeroplane with enough performance to take off and fly without assistance from gravity.

Once the principles of stability and control were understood the emphasis shifted towards performance, and today the basis for design is to achieve some ever-increasing standard of performance, and at the same time to retain acceptable standards of stability and control. To combine these two aims in any practical design involves compromise, and decisions must be made as to how much of each is to be incorporated. To do this efficiently we must know precisely what constitutes good stability and control qualities, and then how to build an aeroplane which will incorporate them.

In this lecture it is proposed then to consider the following:—

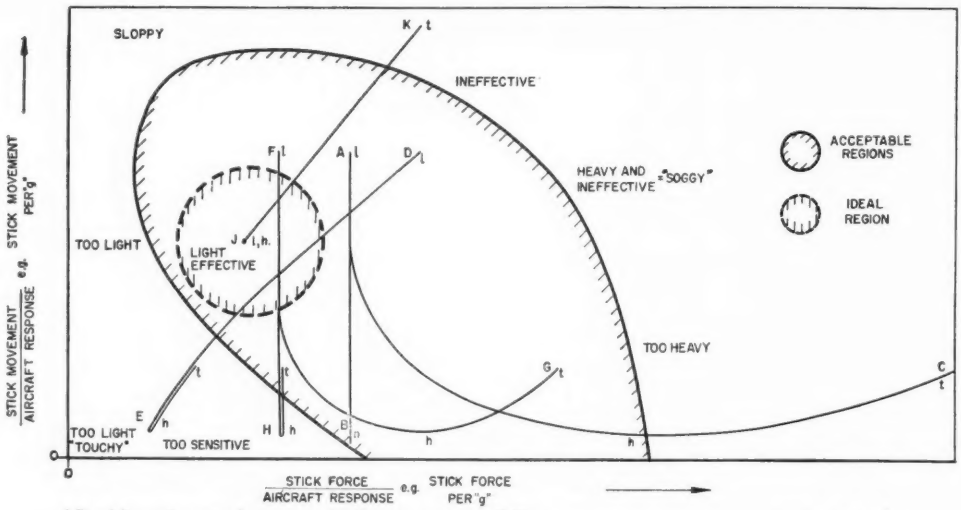
1. Stability and control requirements.

2. Determination of the air forces.

3. Practical design to achieve the desired handling qualities; and finally to suggest the more important gaps in present-day knowledge of the subject.

Notation

b	wing span, feet
$\bar{c}$	wing mean chord, feet
V	true air speed, feet per second
M	Mach number
q	dynamic pressure $=\frac{1}{2}\rho V^2$
R	Reynolds number
"g"	normal acceleration in gravity units
$\overline{V}$	horizontal tail volume
C_M	pitching moment coefficient
H_n	static margin, stick fixed, in fraction of mean chord
H_m	manoeuvre margin, stick fixed, in fraction of mean chord
S.F./g	stick force per "g"
α	wing incidence
ε	downwash angle
ϕ	angle of bank
β	angle of sideslip
ξ	aileron angle
p	rate of roll, radians per second
μ_2	relative air density $=m/(\rho Ss)$ where m=aircraft mass, ρ=air density, S= wing area and s=wing semi-span (Fig. 8)
$\frac{dC_l}{d\xi}$	rolling moment coefficient per radian aileron deflection
$a_1=\frac{dC_L}{d\alpha}$	
$a_2=\frac{dC_L}{d\xi}$	
$m=\frac{dC_M}{d\xi}$	
n_ξ	yawing moment coefficient per radian aileron deflection
l_v	rolling moment coefficient due to sideslip
n_v	yawing moment coefficient due to sideslip
l_r	rolling moment coefficient due to yawing
n_r	yawing moment coefficient due to yawing
l_p	rolling moment coefficient due to rolling
n_p	yawing moment coefficient due to rolling
f	frequency, cycles per second



AB—Manual control, no aeroelastics or compressibility
AC—Manual control with aeroelastics and compressibility
Full power controls:—
DE—Simple spring feel, with aeroelastics and compressibility
FG—"q" force feel
FH—Inertia weight with aeroelastics and compressibility
JK—Spring gearing varying with "q," with aeroelastics and compessibility

l = low speed
h = high subsonic speed
t = transonic speeds

FIGURE 1. Feel diagram.

2. Stability and Control Requirements

2.1. PRELIMINARY CONSIDERATIONS

The pilot is the real nerve centre of any aeroplane; he initiates all movements of the aircraft and expects it to obey. An aeroplane will be said to have good flying qualities if it is reasonably stable when left alone, but responsive and obedient to all the directives from the pilot when he wishes to change its flight path. Thus, to design for good flying qualities is to design an aeroplane whose properties will be suitably matched to the natural capacities of the pilot.

In the early days of flying pilots soon developed their own descriptions of the handling qualities of an aeroplane, and terms such as "heavy," "light," "sloppy," "responsive" and so on came into use. The designer would have to interpret these terms as well as he could, and then attempt to design an aeroplane which incorporated the desired qualities. It is obvious that to do this effectively it is necessary to know precisely what the pilot means by his descriptions; they must be translated into specific numerical terms so that the desired qualities may be designed rationally into the aircraft.

It is largely to the work of Gates[1] that the first comprehensive formulation of requirements for stability and control is owed. Terms such as stick force per "g" gave the designer something he could "get hold of" and really attempt to design into the aeroplane.

However, there is still a great deal that is not known about the make-up of good flying qualities, and new problems continually arise with the present rapid pace of aircraft development. Quite often an aircraft spends a long period of development time working up to some standard of flight behaviour based on the pilot's judgment. In the last analysis his judgment of flying qualities must always be accepted, but if more were known about the make-up of these judgments two important advantages would accrue:—

(1) It would be possible to design sufficient stability and controllability into the aircraft from the start, with no unnecessary ignorance margins and associated performance penalties.

(2) The test flying development period would be shortened.

2.2. THE MAKE-UP OF THE HANDLING QUALITIES

The pilot's judgment of an aeroplane's handling qualities is made up of three elements:—

(*i*) The effort the pilot has to make, consisting almost entirely of stick *forces* and stick *displacements*.

(*ii*) The behaviour of the aeroplane, both when left to itself (its stability) and when the controls are used (its response). This is sensed *visually*, and by feel of the aeroplane's *acceleration* and *response*.

(*iii*) The *time element*, or how quickly or slowly things happen.

The complete judgment is made up of the combination of all these three items, and is thus a fairly complicated quality to describe; it has been usual to consider simple combinations in formulating existing requirements for good handling qualities, *e.g.* stick force per "g," angle of bank per second and so on. It is probable

that any future improvements in understanding of the problem will have to involve all three elements together.

Some immediate design problems will now be covered in which a more searching diagnosis of the situation could lead to improved design methods.

2.3. SOME CURRENT PROBLEMS ON HANDLING QUALITIES

2.3.1. *Control feel requirements*

The problem here is to relate the stick force and stick movement to the response of the aircraft so that the pilot has effective control and can be aware, through the stick, of the forces he is putting into the aeroplane.

High performance aircraft of today are being increasingly fitted with power-operated or servo-tab controls. For all high speed aircraft the trend is towards full power operation with no aerodynamic forces fed back to the pilot at all. This has both good and bad effects in providing proper feel, good in that there should be some opportunity of artificially making the stick forces have any desired value, bad in that this introduces extra mechanical complexity.

"To have the desired values of stick forces and feel," but what are these? Recent lengthy discussion on the subject shows that the answer is far from clear. Past experience must be used but at the same time the subject should be re-examined with an open mind.

To assist in this we may refer to Fig. 1 which could be called a "feel" diagram. In it stick force, stick movement and aircraft response are related and some boundaries of acceptable characteristics drawn in from qualitative considerations. Tentative interpretation of favourite pilot's expressions such as "sloppy," "heavy," "touchy" and so on are also shown.

With power controls it is suggested that the stick force per "g" need not necessarily be constant, but that the lower limits particularly should be made to depend also on stick movement and thus allow for what can be an important modifying factor. The actual positioning of the boundaries, particularly the stick movement boundaries, will vary somewhat with speed and the manoeuvre being considered, but the type of diagram should apply in principle to all manoeuvres.

This feel diagram relates three quantities, but even so it suffers from the serious objection of leaving out the time element. Two aspects of the time response behaviour are known to be very important. Firstly the aircraft must possess acceptable dynamic stability qualities, or every time the controls are used they will excite an unpleasant motion. Secondly it must be possible to move the controls themselves at a rate which is high enough in relation to the natural response time of the aeroplane. Present ideas on desirable rates are very general and vague, *e.g.* about 35°/sec. is aimed at on an elevator and 40 to 50°/sec. on an aileron. These rates have an important influence on the design of power controls and it would be desirable to have a more rational basis for estimating the rate that is really needed and to design for that and no more.

A further important element in feel, which has been brought very much to the fore by power controls, is the need for proportionality or linearity in the feel. Power controls tend to give two basic non-linearities:—

(*i*) Non-linearity in movement, *i.e.* backlash and lost motion.

(*ii*) Non-linearity in forces, *i.e.* preload forces, friction and stiction.

Both of these non-linearities are disliked by pilots and the ideal would be to eliminate them. Unfortunately some non-linearity is almost inevitable in a power control due to the force and movement required to open the booster valve. Further non-linearities may arise on the input side of the control circuit and in the feel unit. Manual controls have had friction, but no backlash; but with power boost these effects on the input side represent uncertainties in the control of a large source of power and are in effect magnified by the booster unit to produce a significant influence on the control of the whole aeroplane.

Flexibility in the input circuit can be very objectionable, particularly if it occurs between the feel unit and the valve, or the trim jack and the valve. The pilot will then not feel exactly what the booster is doing, and the operation of the trim wheel may merely tension up the input control cables instead of operating the booster—this will make trimming of the aircraft extremely difficult. The best solution to this problem is probably to put the feel unit and trim jacks as close as possible to the booster so that they form one rigid system. If this is done the non-linear movements remaining will probably not be serious but the non-linear force characteristics may still be objectionable. Thus a lot of friction in the elevator circuit will necessitate larger pre-loaded feel springs to give self-centring with a resultant large "break out" force. This may be strongly objected to as it makes accurate flying difficult, there being a tendency to overdo all small corrections. Large break out forces may also mean a sharp snap back into neutral when the control is released and are disliked for this reason as well.

Power controls then have provided a number of new considerations on desirable feel qualities. Their solution lies mainly in careful attention to the detailed mechanical design of the control circuit, and in the provision of suitable systems of artificial feel.

More generally there is still a great deal to learn about the make-up of good control feel, in particular the relation between the stick force, movement and the time element. It is probable that a good deal of work could be done analysing the make-up of the feel into these various constituents. This should start on known aircraft with manual controls, and then pass on to known power control systems before finally suggesting the ideal characteristics for future designs.

For some consideration of the various methods of achieving artificial feel, *see* Section 4.4.1(c).

2.3.2. *Dynamic stability problems*

The importance of the time factor in the pilot's judgment of an aeroplane is well brought out in the following problems, most of which involve dynamic stability.

(*a*) *Poorly damped phugoid* (*i.e. speed*) *oscillations*

On clean jet aircraft the damping of the long slow phugoid oscillation is basically low. It is probably not very serious in a day fighter aircraft, but on all other types it can be distinctly irritating to the pilot as the slightest disturbance, whether caused by gusts, temperature changes or passenger movement, will take a long time to damp out. Use of an automatic pilot seems to be the best solution.

(*b*) *Poorly damped pitching oscillations*

This is a quick motion taking place at approximately constant speed. Until fairly recently most aircraft have had a lot of damping and, therefore, a quick subsidence following a disturbance, which is a very acceptable motion from the pilot's viewpoint. A lot of the damping of the motion comes from the tail and is crudely due to the tail "wagging about" whenever the aeroplane pitches. Tailless aeroplanes or aeroplanes with small tails will, therefore, have much lower damping and this may cause a definite periodicity in the movement. This is not liked as the period is usually short (of the order of a second) and, therefore, the pilot can do nothing about controlling the motion, but must leave it to die out naturally. Pilots seem more sensitive to the number of cycles to damp to $\frac{1}{2}$ amplitude than to the absolute damping. Typical variations of this with stability (H_m) are shown in Fig. 2.

It is seen that for a conventional tailed aeroplane the motion is effectively a subsidence at low altitudes, but that at high altitude a certain amount of oscillation or overswing has come in. This is probably at the root of the frequent complaints that nearly all aircraft become "touchy" at high altitude, where the reduced damping comes from the much lower air density. As it appears quite possible that this type of behaviour may determine tail size it would be desirable to know just how much of this loss of damping can be tolerated. Fig. 3 shows the variation in damping with tail volume $\bar{V}$ for a representative high speed aircraft flying at 40,000 ft. On the right hand side of the figures are shown the types of motion corresponding to various damping ratios, and it is seen that to avoid a pronounced oscillatory component the damping ratio should be not less than about 0·40, with an associated $\bar{V}$ of 0·40. This $\bar{V}$ is of the same, or rather greater, order than may be required for other purposes so it appears that damping requirement may be important.

The foregoing describes the normal behaviour and is concerned with unpleasant rather than dangerous characteristics. It is possible, however, for the damping of the short period oscillation to become negative. The motion then takes the form of a rapidly increasing short period oscillation which cannot be controlled by the pilot and which can build up to dangerous amplitudes quickly. In principle this type of motion is caused by lag in the development of the air loads on some part of the aeroplane; such lags can occur at transonic speeds, and also at other speeds if we try to achieve too much stick free stability by excessive use of horn balances and inertia weights.

Divergent short period oscillations are quite unacceptable.

(*c*) *Nose down trim change at transonic speeds*

Under transonic conditions it can be difficult to achieve static stability owing to the fundamental changes in flow regimes between subsonic and supersonic flow. It usually happens, however, that at these Mach numbers the manoeuvre stability is increasing rapidly and so to the pilot the static instability is described as a "gentle nose down trim change"—causing no difficulty if the controls are effective and it is only a temporary flight condition, but, if the static instability occurs within part of the cruising speed range of the aircraft it may be unacceptable, as the pilot cannot trim the aircraft and leave it but must be continually correcting the tendency for the speed to increase.

Acceptable standards of behaviour in the transonic speed range are still under debate but it appears likely that some static instability may be acceptable on day fighters only and that some solution either inherent or by artificial stabilisation will be necessary on other types of aircraft.

(*d*) *Cruising speed—dive speed margins*

To achieve the best possible performance jet-propelled aircraft need to cruise very near to their critical Mach number. This means that for a civil aircraft it can be difficult to achieve the margins between the

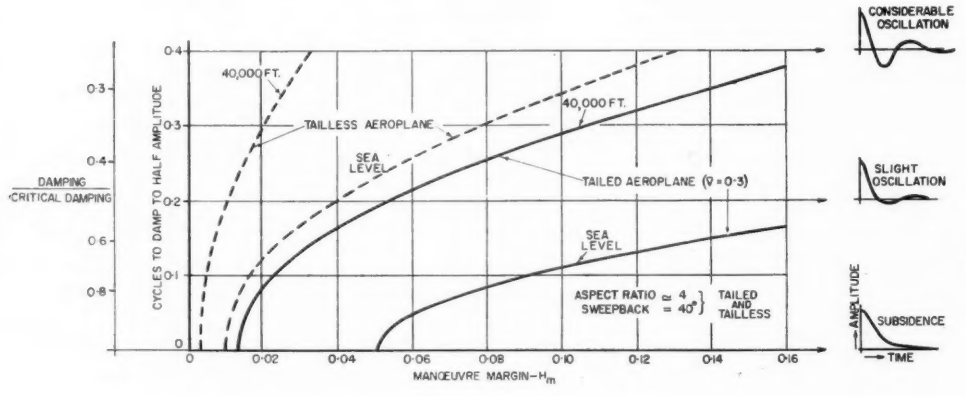


FIGURE 2. Damping of short period oscillation.

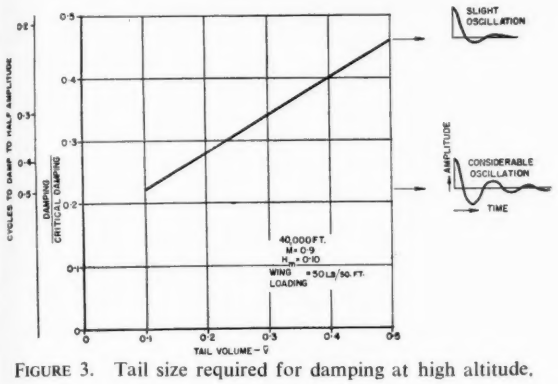


FIGURE 3. Tail size required for damping at high altitude.

cruising and dive speeds which were hitherto required on propeller-driven aeroplanes. These margins originated from statistical analyses of operating experience on the older propeller-driven types and they have to be re-examined for jet aircraft. It appears justifiable to close them up a bit and demand a bit more care from the pilot in not exceeding the limits, and when this is done it seems that the effect of random passenger movements on the aeroplanes' speed can become important. Fig. 4 shows the effect of two passengers moving quickly up the length of the cabin (a fairly frequent occurrence). It can be seen that the speed changes can be appreciable and this consideration may determine the amount of static stability margin required on the aft c.g. limit.

As discussed in Section 4.3.2(*b*), the provision of a nose-up trim change at some suitable Mach number is the best way of justifying reduced speed margins.

(*e*) *Directional and lateral stability*

Spiral Stability

In the past it has been fairly easy to ensure spiral stability under cruising conditions by providing sufficient dihedral, and the spiral instability which often occurred at high C_L's only has not appeared to matter a great deal as any disturbance can be corrected very easily by the pilot. However, on aircraft with swept-back wings it is becoming increasingly difficult to obtain spiral stability without increasing oscillatory instability, so it is necessary to decide more precisely what standard of spiral stability is required. Unless reliance is to be placed on an automatic pilot it would still appear desirable to insist on spiral stability on the cruise where the aeroplane will usually have to fly for considerable periods, as an aeroplane which is spirally unstable will persist in wandering slowly off into a spiral dive and will prove extremely irritating to fly except for a short time. Marginal spiral stability will also cause trouble in accurately trimming the aircraft laterally, particularly with power-operated ailerons.

Oscillatory Instability

With sweepback, high wing loadings and at high altitude, it is also becoming increasingly difficult to achieve satisfactory stability of the directional and lateral oscillation. From the pilot's viewpoint a poorly damped lateral oscillation (often called "Dutch Roll") is noticed particularly in rough air where the aircraft never settles down from one gust before being upset by another. This can be either merely unpleasant, or possibly dangerous if on an instrument approach when proper readings cannot be taken, besides being a continual distraction to the pilot. On fighter aircraft it can seriously interfere with gun aiming accuracy and thus negative the whole *raison d'être* of the aircraft.

Automatic Stabilisation

There is a growing practice to cure this oscillatory instability artificially by the use of yaw dampers; the stability can then be greatly increased, indeed probably there is an opportunity of producing far better characteristics than were practicable by inherent stability.

The introduction of artificial stabilisation and auto-pilots also introduces a new philosophy of design for safe flight characteristics. Instead of having to strike one uneasy compromise between conflicting requirements, it introduces a welcome flexibility and enables us to work to two standards:—

1. When the automatic stabiliser is working.
2. When it has failed (a condition that must be allowed for at present).

Automatic stabilisers and auto-pilots can effect an enormous amount of stabilisation compared to what is possible by natural methods and we can then design to these two standards; one the high standard of what we would really like when the automatic stabiliser is working; the other the much lower one of the minimum acceptable emergency standard, necessary for safety and not convenience, when it has failed.

(*f*) *Low speed aileron and elevator control*

The important parameter in the low speed aileron control is the helix, or "screw" angle the aircraft makes when the aileron is deflected.

$$\text{Helix angle} = \frac{pb}{2V},$$

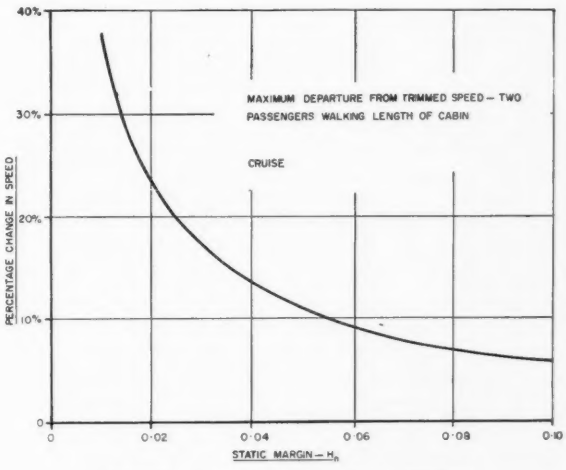

MAXIMUM DEPARTURE FROM TRIMMED SPEED — TWO PASSENGERS WALKING LENGTH OF CABIN
CRUISE
PERCENTAGE CHANGE IN SPEED
STATIC MARGIN — H_n

FIGURE 4. Effect of passenger movement.

See also Discussion—reply to Dr. Doetsch.

when in steady roll, and current requirements are that for normal aircraft

$$\frac{pb}{2V} \leqslant 0{\cdot}07$$

and for naval aircraft

$$\frac{pb}{2V} \leqslant 0{\cdot}09.$$

On the approach aileron is used to pick up a wing if it has dropped because of a gust, and to enable the pilot to make the desired corrections to align himself on the runway. It is not too clear why the helix angle is the correct measure of these two qualities, and the chief justification for using it as a design parameter is that it seems to work.

Now $$\frac{pb}{2V} \propto \frac{\frac{dC_l}{d\xi} \times \xi}{I_p}$$

and therefore for a given aileron to wing proportion $pb/(2V)$ is independent of aircraft size. For a large aircraft this means that the actual rate of roll p will be lower than on a small aircraft. That this is acceptable is indeed fortunate, but why is it? For the case of picking up a wing after a gust it may be understood because a given gustiness will probably upset the big aircraft less and, therefore, less gust correction is required. For making alignment corrections the implication is that the pilot is content with a slower correction on the larger aircraft, but why this should be so is not clear.

As with the elevator control the response should be smooth and good stability characteristics (*e.g.* absence of pronounced oscillatory components) will enhance this quality. Acceleration into the roll must also play a part and cases are known where a pilot has adversely criticised aircraft with a lot of fuel near the wing tips with consequent high rolling inertia and poorer acceleration into the roll.

It is, therefore, probable that the $pb/(2V)$ criterion is rather crude and some more comprehensive criterion related to the pilot's ability to execute "S" turns in a given distance may be more rational.

A related problem on the fore and aft control concerns the pilot's ability to approach, flare out and land. The ease of doing this depends on a complex mixture of the fore and aft stability and control characteristics, the speed of the aircraft and the lift/drag ratio. A propeller-driven aircraft has a potent means of controlling the flight path on the approach as there are large and immediate changes in both lift and drag when the engine power is changed. On a jet aircraft change in engine power produces little change in lift, there is no drag as from a windmilling propeller, and the response to throttle movements is much slower. The pilot must then control his flight path mainly by use of the elevator; he has virtually lost the extra drag and lift control which the propeller used to give him. There are indications that to make a good landing the jet aircraft does in fact make greater demands on the pilot's judgment, and to offset this a progressive air brake under the pilot's direct control has been suggested. Against this it appears that some types of aircraft, the crescent wing in particular, have an inherent nose up trim change as the ground is approached and will therefore execute automatic landings; such characteristics may offset the disadvantages of the absence of propellers.

This situation is typical of several control response problems; the designer is left with the feeling that he does not quite know what is going on. It seems that by far the biggest doubt is about the pilot's judgment on anything involving changes with time—he uses terms such as "crisp," "soggy" to describe these time effects. However, short of knowing that he must make some change to deal with an adverse criticism, the designer does not always know how much to change and cannot be sure of guarding against a repetition of the same trouble in a future design.

2.4. CONCLUDING REMARKS

To gain more insight into the make-up of good handling qualities more measurements of the stability and control behaviour under dynamic conditions are required, and more effective methods of analysing such measurements.

It has been suggested[2] that the pilot/aeroplane combination is really a complete but complex form of servo mechanism, and as such should be treated by the now well established methods of servo control analysis. The main difficulty is in pinning down the characteristics of the pilot, as he is such a complicated and adaptable object, but the method holds considerable promise in the future for putting our understanding of the make-up of good handling qualities on a firmer basis, because it correlates the pilot, the aeroplane and the all-important time element.

3. Determination of Air Forces

3.1. GENERAL CONSIDERATIONS

Before an aircraft can be built to incorporate the desired handling qualities it is necessary to be able to estimate the air forces on the various parts of the aeroplane.

Theory aims at understanding and in this lies its great merit; if we understand we can extend or generalise and extrapolate where we should merely guess if we had only experimental results. To gain this understanding theory often becomes very complicated indeed, much too complicated to have much possibility of practical application; however, this complication has one practical function in giving a standard of reference which can then be used as a standard with which to compare simple, more approximate methods. A good example of this process is the current work on estimation of load distributions, where various forms of lifting line theory are compared with the more accurate lifting surface theories to assess their value.

All this theoretical work must be backed by continual comparison with experiment. Fundamental experiments involving detailed study of the flow by pressure plotting, boundary layer traverses, shock wave observation and so on, are essential in helping towards an understanding of the flow, but owing to their length

cannot be made on many configurations. However, there is always a demand for a less detailed knowledge in some basic overall results and there are a vast number of experiments continually being done to help supply this insatiable need. It is here that the simpler, more approximate, theories come into great use as they give, as it were, a blank framework on which to place the experimental results, and a yardstick with which to correlate them.

Generally speaking the aircraft designer has been forced to concentrate more on the simpler overall results, but he is tending now to do more thorough investigations, particularly with the advent of computing machinery.

3.2. WIND TUNNEL TESTS

The great bulk of experimental data on stability and control derivatives is obtained from wind tunnel tests. The wind tunnel is an invaluable tool for this purpose, the main doubt about the results being how much is the scale effect.

Ad Hoc Model Tests

Some conclusions from experience at de Havilland's on *ad hoc* model tests of complete aircraft may be of interest:

(*i*) Model tests will show the main trends in behaviour, in particular showing up any unexpected features in the design.

(*ii*) Apart from effects involving flow separation they are of value at low Reynolds number, the main use of high R being to increase the absolute accuracy of the results rather than show new phenomena. Comparing full-scale results with model tests at $R \simeq 1 \times 10^6$ suggests the following trends:

(*a*) The full scale aeroplane tends to be a little less stable, to the order of 0·02 to 0·03$\bar{c}$.

(*b*) Rather lower control powers full scale.

(*c*) High Mach number trim changes are predicted qualitatively by the model (provided that it has a rear sting support only), but the magnitude and M at which they occur full scale may differ considerably.

(*d*) On the Hornet the full scale aeroplane had much less control power with ground than the model tests with ground showed. We are accordingly suspicious of model tests with ground, which is inconvenient because the elevator power with ground can be a critical design case.

(*iii*) Effects involving flow separation, whether at the stall or due to shock waves, will be much less reliably indicated, but even so it does not pay to ignore the indications of the tunnel. The use of partial models, and any means of increasing R for such tests is of course very important.

3.3. SOME MORE IMPORTANT DERIVATIVES

3.3.1. *Pressure and load distributions*

Recently developed methods are showing encouraging results, and it is becoming practicable to use lifting surface calculations for estimating pressure distributions over a wide range of profiles and plan forms up to subcritical Mach numbers. This work has been done by close integration of theoretical and experimental methods. While not used in the project stage it is now coming into increasing use in the design stage. In fact, when tackled systematically the use of these methods is not so serious from the point of view of excessive labour of computation, and it is found that the improved accuracy of the methods is well worth while.

At supersonic speeds there is much more theory than experiment to go on and experience so far suggests that the simpler linearised theory, while of considerable value in providing the yardstick for correlating experimental work, is not in itself sufficiently accurate to be relied upon and the last ounce of benefit has to be extracted from the much more limited experimental data.

Pressure distributions are very valuable in following and understanding the development of the stall and, combined with flow studies, form the only satisfactory way of gaining some insight into a very difficult branch of aerodynamics.

Finally, pressure and load distributions supply information to enable the designer to make the necessary aeroelastic calculations. Such calculations involve structural and aerodynamic parameters, but their accuracy is limited mostly by doubts of the aerodynamic derivatives used, and any wind tunnel tests giving pressure distributions, especially with controls deflected, are most useful.

3.3.2. *Control powers*

The position here is less satisfactory. A considerable amount of data exists on the two-dimensional subsonic control, but not so much as might be desired for the three-dimensional case. At supersonic speeds the plain flap shows the large drop as predicted by theory and even more so.

It is common for a control to be used to at least 25° and quite often up to 30° or more. At such angles there will be a certain amount of flow separation, particularly at low tunnel Reynolds numbers. As the low speed, large angle condition often decides the size of controls it is unfortunate that there is not more accurate data on this.

3.3.3. *Control hinge moments*

Of all the derivatives these have proved among the most difficult to estimate and a continual source of trouble in the development of an aeroplane.

A most valuable contribution to the understanding and systematisation of hinge moments was made several years ago by Bryant; as a result of this a reasonably reliable estimate of hinge moments on plain two-dimensional controls became possible. A major conclusion was that low trailing edge angles are essential to obtain repeatable hinge moments—a conclusion which both model and flight tests have shown applies with even more force at high subsonic Mach numbers.

Unfortunately the combination of radically changing plan forms, transonic and supersonic speeds, means that there is again a considerable gap in knowledge.

It had been thought that with the use of power controls the need for accurate knowledge of hinge moments would lessen; the provision of adequate power would do by brute force what had before to be done by finesse. However, experience shows that plain unbalanced controls need large heavy boosters to push them over, and it is more economical in weight to have some moderate aerodynamic balance and a small booster. Thus there will always be the need for fairly accurate knowledge of the hinge moments before designing a power control.

3.3.4. *Directional and lateral derivatives*

Until recently it has been usual for there to be a fairly wide range of values of l_v and n_v which would produce satisfactory stability. However, with recent developments the region of stability has steadily shrunk and may even be non-existent at high altitudes. Thus it becomes necessary to know all the important lateral derivatives more accurately than before, the following in particular:

l_v, n_v, l_r, n_p, n_r (particularly n_r body)

and to know their variation with lift coefficient and Mach number.

Data relevant to current design needs is very limited indeed, particularly at high Mach number, and there is a need for more experimental and theoretical data to supply even a skeleton outline.

3.3.5. *Oscillatory derivatives*

These are becoming increasingly important for two reasons.

(*i*) The increase in problems of dynamic stability.
(*ii*) Flutter.

Until recently dynamic stability calculations were always made using quasi-static derivatives, *i.e.* making no allowance for time lags in the development of circulation round an aerofoil. Recent flight experience is showing a need for increased attention to problems of dynamic stability, both longitudinal and lateral, and it appears that increasing attention must be given to the use of true dynamic derivatives. A closely related problem is that of the transient lift development during a gust.

Now unsteady flow effects are dependent on frequency parameter

$$\nu = \frac{2\pi f c}{V}.$$

This can reach values of 0·1 to 0·2 in dynamic stability motions when significant changes in derivatives from their quasi-static values are possible, particularly at transonic speeds. Experience shows that the most important effects exist at transonic speeds and can affect either the whole aircraft or the controls only. As with most transonic manifestations, conditions seem to be radically improved by making aerofoils thinner and as this is an inevitable trend for performance reasons it is hoped that future troubles may not be too serious. Fully irreversible power controls adjacent to the surface should stand a very good chance of preventing any control surface oscillations or "buzz." We have found that on a free control, the trailing edge strip, which is so often used to avoid over-balance, can cause a buzz of the control at high Mach number.

3.3.6. *Flutter*

Although not under the strict title of stability and control it should be noted in passing that it is in this closely related field that there is the greatest need for more knowledge of the oscillatory aerodynamic derivatives.

3.4. DERIVATIVES FROM FULL SCALE FLIGHT TESTS

In Industry flight testing is done with the prime object of developing the aircraft and then showing that the aircraft's behaviour is safe and to the required standards. Thus while measurements will be taken of the final answer, *e.g.* SF/g, rate of roll and so on, it is not usually possible to deduce much fundamental information on derivatives within the time available for testing. It has been shown by Milliken[3] that many derivatives can be deduced from response measurements and it is possible that this method could be applied to the analysis of much flight data. Instead of always having to conduct special and lengthy tests to determine stability characteristics it could potentially yield a large amount of information from records of a pilot's ordinary manoeuvres. This information would be of low accuracy; but it may be that if analysed statistically with electronic computing aids it may give data on many combinations of C_L, M and altitude which, to cover by deliberate testing, would involve a fantastic amount of special test flying such as might only be undertaken by a research establishment.

3.5. DERIVATIVES FROM FREE FLIGHT MODELS

This method has been developed as a means of overcoming the difficulties of obtaining wind tunnel results at transonic speeds. It is well adapted to the measurement of the oscillatory derivatives but a good idea of the steady case derivatives can be obtained as well. The technique is still in its early stages but promises well and, providing that sufficiently accurate instrumentation is available, it should prove a valuable source of data. Apart from obtaining derivatives the method has an inherent attraction in that it automatically shows up any undesirable oscillations or dynamic instability. Thus if a dynamic model of a proposed design behaves satisfactorily the designer has a welcome indication that the full scale aeroplane may also be satisfactory.

3.6. CONCLUSION

The knowledge of aerodynamic forces always lags same way behind the current design and the designer often has to make shrewd estimates from a small amount of concrete data. Some very good aeroplanes have been made by this means, but as speeds and complication increase it is becoming more and more difficult for the designer to feel his way through the variables of speed, Mach number and altitude. Continual research effort on the aerodynamic derivatives is needed to enable him to retain adequate certainty of the success of a new design, and to lessen the lengthy and expensive period of test flying.

4. Stability and Control in Design

4.1. INTRODUCTION

Assuming some knowledge of stability and control requirements and the air forces, the main task still remains of designing the actual aeroplane to incorporate good stability and control qualities without excessive penalties in performance. All aeroplanes are designed with one sole object; to achieve the best possible performance in the role for which they are intended. Thus in the design stage continual compromises are involved with the other requirements of low weight, low drag and structural and practical mechanical considerations, and the standard of stability and control provided must be sufficient to ensure that the aeroplane can fulfil its prime purpose with safety and precision, but no more.

In examining a new design two methods proceed side by side; one the "explicit" method of evaluating the expected behaviour in detail, and the other the "overall" method in which simple criteria and experience are used.

With increasing speeds and complication the explicit method must be used more and more and in this category come items such as response to control and dynamic stability estimates, detailed aeroelastic calculations, flutter calculations, lifting surface estimates of the air load distributions. These calculations aim at improving the certainty and refinement of the design, but they involve complicated mathematics and this has led to the use of analogue and digital computors. With these aids we are in a much more powerful position to evaluate the optimum design; and with much of the mathematical donkey work thus removed the main doubt about such calculations is that the final answer depends entirely on the accuracy of the aerodynamic force data fed in at the beginning; thus to match the improved computing powers better knowledge is needed of the aerodynamic forces. Nevertheless there can be no doubt that these advanced methods are here to stay. They will not do our thinking for us; instead, by evaluating many cases they can give us much more to think about in the design stages. Used sensibly, a better design should result.

At the same time it is essential to retain something of the overall method, and broad yardsticks such as tail volume, tail arm and so on, should be reviewed in relation to past experience to make sure that in the complex evaluation we have not lost sight of the wood for the trees.

With these considerations in mind a few present outstanding problems will be discussed.

4.2. TAIL DESIGN

4.2.1. *The horizontal tail*

This is used to provide:—

Fore and aft control.

Fore and aft damping

To augment fore and aft static and manoeuvre stability.

4.2.1.1. *Fore and aft control*

The size of tail is frequently governed by the requirements of low speed control at take-off and landing. At these speeds the control has to overcome the moments arising from the following sources:—

(*i*) The adverse pitching moment from the flaps. This will be large if high lift flaps are fitted and much smaller with simple flaps.

(*ii*) The stability in the landing condition. This will depend on the c.g. travel and also to a considerable extent on whether or not the aeroplane is propeller- or jet-driven. If it is propeller-driven there will be an added stability when the engines are throttled back at landing due to the absence of the large destabilising slipstream effect which is present in normal powered flight. The corresponding effect of power on a jet is much smaller. Thus there will be less excess stability when landing on a jet-propelled aeroplane, and, other things being equal, some reduction in the tail size required for landing such an aeroplane may be expected.

The All-moving Tail

At transonic and supersonic speeds fore and aft manoeuvrability is reduced for two reasons; there is an increase in stability, and a large reduction in the effectiveness of the normal elevator. The answer to this problem is to have the tailplane movable and, for best results, the whole tailplane should be directly operated by the stick. This all-moving tail also represents an advantage in low speed flight. Under the landing condition in the presence of the ground there is a considerable positive incidence on the tailplane providing an upload, which is offset by a larger up elevator deflection giving a download. This strongly suggests that a very large increase in control could be obtained if the tailplane were to be adjusted to a negative angle for landing. Until recently this was regarded as too much of a mechanical problem to be practical, but the requirements of adequate control at transonic and supersonic speeds are forcing us to the use of movable tailplanes for high speed flight, and there is then a strong case also for using them for landing, thereby reducing the size of tail needed for that.

The all-moving tail does of course introduce its own new problems, of which the most obvious are the increased operating power requirements, the virtual impossibility of any manual control, and the vital need of preserving adequate stiffness as a safeguard against flutter and aeroelastic difficulties. There is some advantage structurally in that the tailplane and elevator are not so heavily stressed by large torques, as at high speeds the elevator is neutral. On balance it appears that the fully power-operated all-moving tailplane with or without a geared elevator is an attractive proposition for both high speed *and* low speed control, and an aeroplane more efficient in both performance and control should result from its use.

As an illustration of the influence of landing requirements, Fig. 5 shows some approximate estimates of the

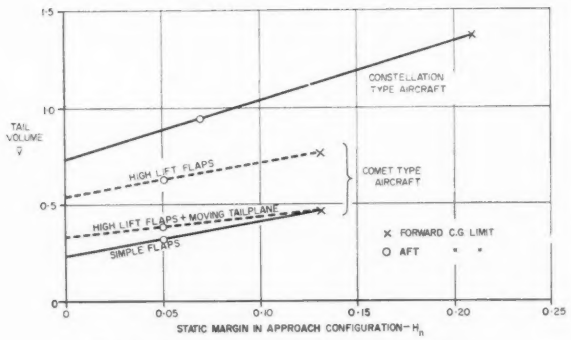


FIGURE 5. Tail size required for landing (estimated by approximate method).

tail volume required to land two civil transport aircraft, one representative of the four-engined propeller driven type, such as a Constellation or DC-6; the other the first of the jet propelled civil aircraft, the Comet. The tail volume on the Comet was determined largely by landing control and its value, 0·485, is low for a civil aeroplane, being less than half that of the Constellation whose $\bar{V}$ is 1·11. This low tail volume represents one of the secondary advantages of jet propulsion, and contributes materially to the overall performance (*see* Section 4.2.3). To see why it is low, Fig. 5 shows the variation of $\bar{V}$ required as one by one the different factors between the two types are made alike. It is seen that about half the difference is due to the simpler flaps on the Comet, and half to the smaller variation of stability with flight condition on the Comet, and the smaller c.g. range. The value of the all-moving tailplane with elevator is apparent in that if the Comet were to be fitted with high lift flaps the $\bar{V}$ required with a normal tail would be about 0·77, which would be reduced to 0·46 with 10° of negative tailplane movement.

4.2.1.2. *Damping in pitch*

At transonic speeds there are doubts about the wing damping, due partly to the general pattern of transonic flow, and partly to shock wave boundary layer interaction and associated flow break downs. The problem is obviously most serious on a tailless aeroplane, and for this reason a reasonable size tail ($\bar{V}$ not less than about 0·20) is regarded as very desirable on a transonic aircraft, at least until more is known about the problem.

4.2.1.3. *Effect of tail on static and manoeuvre stability*

It might be thought at first sight that this was the main function of the horizontal tail and should have been dealt with first. However, this is not so, the requirements of control and damping exert a more fundamental influence on tail size, and having decided on the size necessary for these purposes the provision of adequate static and manoeuvre stability should be done ideally by adjusting the position of the c.g. to be the required distance ahead of the neutral and manoeuvre points. At least so one reasons in the design stage, but it must be admitted that aeroplanes have sometimes been found to be deficient in basic stability when test flown, or the c.g. has drifted back, and then the only practical course may be to increase the tail size. Recently it has been our practice to have two tolerances in the aeroplane in the design stage; one of $0{\cdot}02\bar{c}$ for errors in stability estimation, and one of $0{\cdot}02\bar{c}$ for errors in c.g. determination. If the aircraft and c.g. come out as anticipated then there is some margin which can always be used in later developments, but if the aircraft comes out less stable there is less likelihood of major modifications being necessary in the prototype stage. The usual system of careful weight and c.g. control during the design stages is of course very valuable in preventing the c.g. from going outside the original design limits.

4.2.1.4. *General trends in tail size*

In Fig. 6 are plotted the tail volume coefficients of a number of successful aircraft covering the past 15 years. It is seen that the values fall naturally into groups; the largest tail volumes are on propeller-driven civil aircraft fitted with high lift flaps, then propeller-driven aircraft with simple flaps, jet propelled aircraft with high lift flaps, to the lowest tail volumes on the jet fighter with simple flaps. These trends confirm that the tail size is often governed by considerations of low speed control; only in the latter group of jet fighters with simple flaps may the tail volume be determined by other considerations, in that case possibly by damping requirements.

It has been inferred that the large tail volumes on American civil aircraft imply that they use bigger stability margins than we in this country. This may be partly true, but it seems that the main purpose of their large tails is to trim out the high lift flaps and to enable a large c.g. range to be used.

4.2.1.5. *Position of the horizontal tail*

For straight-wing aircraft the downwash varies relatively little with change of tail height. Swept-back

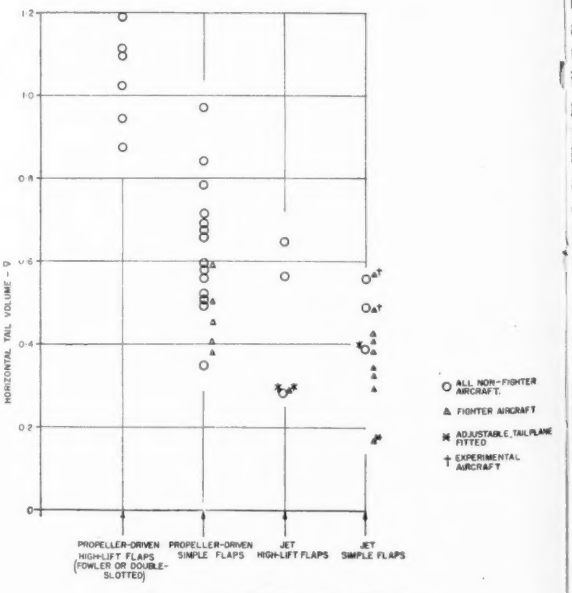


FIGURE 6. Horizontal tail sizes.

FIGURE 7. D.H. Heron.

wings tend to stall first at the tips and thus at high incidence there can be a rapid increase in downwash due to the concentration of lift over a smaller effective span, indeed $d\varepsilon/d\alpha$ may then exceed unity, when the tail contribution becomes destabilising. The effect grows rapidly as the tail is moved above the wing chord-plane, and then decreases gradually as the tail is placed really high. As the wing also will become less stable under these conditions, the two effects may combine to cause a really dangerous nose up instability at the stall. There are two cures for this; either to eliminate the wing tip stall, or to place the tail in a position which avoids the region of high downwash. The former would seem to be the preferable course as it is often desirable to avoid a wing tip stall to preserve good lateral qualities at the stall, but failing this it is probably necessary to place the tail where it avoids high downwash. A tail-plane below the wing chord line will completely avoid the high downwash region, but that in turn brings associated practical difficulties of ground clearance, and unless an all-moving tailplane is fitted it could lead to increased control difficulties in the presence of the ground. Alternatively, the tail should be as small as possible to minimise the destabilising effect, when it may be placed either very high (on top of the fin) or *just* above the wing chord line where it may be just high enough to avoid the high-speed wing wake and just low enough to avoid the high downwash region at high incidence.

Slipstream effects on stability depend considerably upon tail position. Slipstream usually has only moderate effects on manoeuvre stability, but may have very large adverse effects on static stability. The effect seems to be worst when the tail is in the upper part of the slipstream, decreasing both above and below this position. For example, the prototype Heron first flew with a conventional tail mounted on the top of the fuselage and had serious static instability on the climb. Both stick fixed and stick free, neutral points were in front of $0{\cdot}20\bar{c}$, *i.e.* in front of the forward c.g. limit! Some attempts were made to cure it stick free by adjusting horn sizes and trying asymmetric horns, but these effected only minor improvements. It was decided that the only real cure would be to get the tailplane out of the slipstream and a 13° dihedral tailplane was fitted (*see* Fig. 7). This immediately cured the trouble and the Heron is now satisfactorily stable on the climb back to the aft limit of $0{\cdot}38\bar{c}$, although even now the power on climb is the condition of least stability.

As it is quite difficult to find a suitable position for the rear tail there seems to be a good case for looking into the tail-first or Canard arrangement for a high-speed aircraft.

4.2.1.6. *Tail design as affected by aeroelasticity*

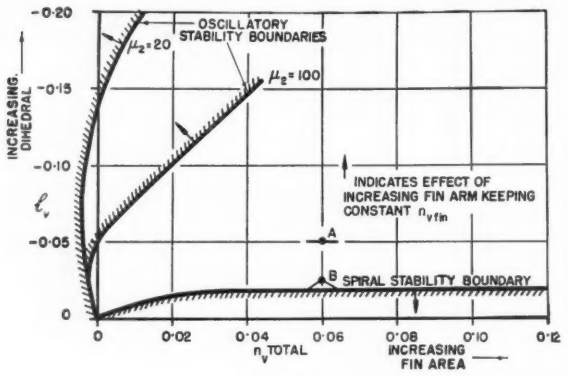
Stiffness criteria for tailplanes, elevators and fuselages were introduced in 1943. These were based on a statistical analysis of existing types and served to indicated a fairly safe level of stiffness to avoid large effects on fore and aft stability and control. However, as the effects are so interwoven it can prove unnecessarily extravagant in weight to satisfy all the criteria at the same time, and it is a good practice to work each case out fully on its own merits. On the Comet, for example, the fuselage is inherently stiffer than the criterion demands and it is possible to balance off this excess stiffness by reducing the tailplane stiffness. This has been done and though the tailplane does not meet the criteria by quite a long way, calculations show that the actual overall combination of tailplane, elevator and fuselage gives a similar standard of elevator control at high speed as if all the components met their respective criteria individually.

4.2.2. *The vertical tail*

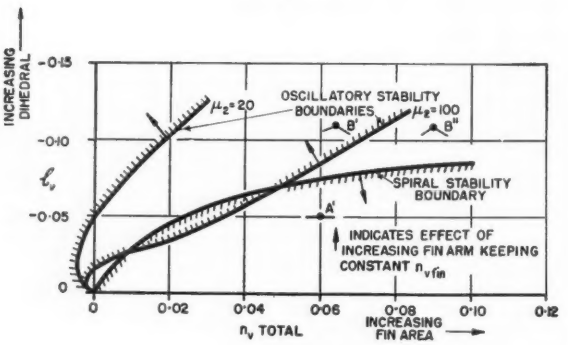
For straight-wing propeller-driven aircraft satisfactory directional and lateral stability can usually be achieved by satisfying the two conditions following:—

(*a*) First provide fin size so that n_v is not less than $0{\cdot}04 - 0{\cdot}05$, and preferably not less than $0{\cdot}07 - 0{\cdot}08$.

(*b*) Then provide dihedral angle sufficient to give $-l_v/n_v$ approximately equal to 0·75 to 1·0.



(*a*) C_L=0·1



(*b*) C_L=0·5

FIGURE 8. Lateral stability diagram.

Having satisfied these simple conditions it will usually be found that the dynamic stability will be satisfactory, and a typical result is shown as the points AA′ on Fig. 8. It is seen that l_v and n_v do not vary appreciably with C_L, but the position of the stability boundaries do, so that it is relatively easy to provide complete dynamic stability at cruising C_L's, but much more difficult at high C_L's when the two boundaries close in together. For the point shown the aeroplane would have a damped oscillation at all C_L's, but would be spirally unstable at high C_L's, which is usually acceptable.

Recent trends in design make the problem much more difficult. The clean jet aircraft with no nacelles or propellers, the swept-back wing, high altitudes and high-wing loadings, all combine in an adverse sense, and it is difficult to achieve qualities as good as on the older, more conventional types.

The swept-back wing behaves like a straight wing whose dihedral is increasing in proportion to incidence. This effect is very large, *e.g.* a 40° swept wing with zero geometric dihedral will have a dihedral effect at a C_L of 1·0 equivalent to 12° of dihedral on a similar straight wing. The points BB′ on Fig. 8 show how, as a result, the swept wing aircraft could easily cross the oscillatory stability boundary as C_L is increased.

The foregoing effects combine to give the worst conditions either on the climb at high altitude or on the low-speed approach condition.

What practical steps can the designer take to meet this situation? Assuming to start with that he wishes to provide as much inherent stability as possible there are three main variables to operate with:—

Fin arm,
Fin size,
Wing geometric dihedral angle.

To show these effects more clearly the usual lateral stability diagram has been presented in Fig. 8 to show how the stability boundaries vary with these three quantities. It can be seen that at low C_L the region of complete stability is fairly large, but at high C_L it is much smaller, and the requirements for spiral and oscillatory stability may then conflict. It is usual to design for a bare minimum of spiral stability on the cruise only, and to concentrate the main effort on improving the oscillatory stability.

Increase of fin arm will increase n_r and so will benefit both oscillatory and spiral stability. It is therefore desirable to do this, but other design considerations soon set a limit to the amount that is practical. Increased fuselage length means considerable increase of weight, particularly if the jet pipe is within the fuselage. Even more serious is the limitation of ground angle; on low aspect ratio swept wings large incidences have to be used for take-off and landing and a long fuselage would then necessitate a very long undercarriage, with the obvious associated structural and stowage problems. There is thus a fairly well defined practical limit to the fin arm that can be provided (incidentally this limitation is equally embarrassing on the horizontal tail arm).

Increasing fin size will improve the oscillatory stability and have only a small effect on spiral stability (*see* points B′B″ on Fig. 8). Fin size can be increased considerably if so desired, but performance and structural considerations must be borne in mind.

*The dihedral may be chosen to just give spiral stability on the cruise, which may mean spiral instability either at high C_L or at C_L=0.

Providing sufficient inherent stability has proved difficult and many aircraft are using artificial stabilisation of the oscillatory mode by installing a yaw damper. This device uses a gyroscope and amplifier which move the rudder in the correct phase to give a big increase in the direct yaw damping term n_r.

The aim then becomes to design an aeroplane with inherent oscillatory characteristics which are just safe when the yaw damper is inoperative, and to use the damper to increase the damping to the required standard for normal operations.

4.2.3. *Comparison of Comet with Constellation*

The tail volumes of the two aircraft compare as follows:—

	Horizontal Tail Volume (Gross)	*Vertical Tail Volume (Net)*
Constellation	1·12	0·064
Comet	0·486	0·023

*See Discussion.

FIGURE 9(*a*). D.H. Comet.

Figure 9 shows photographs of the two aircraft. If the Comet had nacelles and propellers then both the vertical and horizontal tails would have to be considerably larger. The combined increase in area would be about 65 per cent., and if the Comet had needed as large a tail as this the associated weight and drag increase would reduce the payload for a given range by about 18 per cent. of the capacity payload. This illustrates forcibly the large gains in performance to be had on a jet propelled aeroplane by keeping the tail as small as possible.

4.2.4. *Tailless aeroplanes*

The tailless aeroplane received a new impetus at the end of the war when the swept-back wing was recognised as having important advantages for high-speed flight. With the swept-back wing the wing tips effectively became the "horizontal tail," and with sweepbacks of the order of 40° the "tail" arm was sufficiently great to give at least some significant contribution to fore and aft control and damping. Add to these arguments a general desire to get rid of the tail which cannot then give any trouble, and the possible

FIGURE 9(*b*). Lockheed Super Constellation.

FIGURE 10. D.H. 108 tailless aircraft.

gains in performance, and there was the basis for the design of the D.H.108, the first of the modern generation of high-speed tailless aircraft (Fig. 10). It was a research aircraft designed to give information relevant to possible civil and military developments.

Incidentally the D.H.108 is a tailless aeroplane only by lack of the horizontal tail; this is true of most "tailless" aeroplanes as all attempts to dispense with the vertical tail have been virtually unsuccessful.

From the start the flight tests went smoothly and were free from most of the tiresome troubles such as buffeting, and so on, which so often beset a tailed aeroplane. At all normal speeds there was the expected lower damping in pitch, and a relatively small usable c.g. travel. This fore and aft behaviour, however, was acceptable once the pilot had got used to it.

The limits of the aircraft were reached at transonic speeds due to the damping in pitch and fore and aft control behaviour.

Lateral stability was good at low C_L, but at high C_L on the approach there was a slightly divergent oscillation. As this was of a long period (4·25 sec.) the pilot could control it and did not complain about it, indeed it was not really apparent unless deliberately excited and measured.

It was concluded that the type held considerable promise, but needed more development. For a civil type the penalties in c.g. travel, trimmed $C_{L\,max}$ and the less orthodox handling qualities, rendered immediate civil development too uncertain.

As a potential fighter the aircraft appeared quite attractive up to high subsonic speeds, but because of its transonic behaviour the extra damping and control of a tail were considered to be necessary. It was decided, however, to bear the tailless aeroplane in mind for the future when a bit more might be known about it; in particular its damping and control characteristics at transonic speeds.

It now appears that the damping in pitch of the tailless aeroplane will be increased, certainly at subsonic speeds, if there is a large wing chord. This is achieved on the low aspect ratio delta configuration, which may then provide a good deal of the basic damping which the earlier tailless aeroplanes of higher aspect ratio lacked. This would help to overcome the most persistent objection on the handling qualities of the tailless aeroplane, the intrinsically low damping in pitch. Low c.g. travel as a percentage of the mean chord may not be so serious practically on a delta aircraft as it represents a sufficiently large actual dimension when expressed as an actual distance in feet.

With the advent of artificial damping devices it might well be possible to improve a damping which is just uncomfortably low, to desirable values (it would not of course be advisable to do this if the basic damping were dangerously low). This may open up a rather wider range of tailless plan forms.

If the tailless delta were to be considered for supersonic speeds two problems would have to be settled. would there be adequate damping in pitch, and would the conventional elevon or even the all-moving tip supply the requisite manoeuvring ability?

4.3. STABILITY AND CONTROL ASPECTS OF WING DESIGN

Three radically different plan forms are being evolved to meet the current need for long range high-speed aircraft.

These are:—

1. The straight or slightly cranked swept wing of high aspect ratio—subdivided into those with buried engines, such as the Valiant and Comet; and those with pod type engines, the American B-47, B-52 and YB-60.
2. The Crescent wing—The Handley Page Victor.
3. The Delta wing—The Avro Vulcan (and family).

Performance considerations play a large part in deciding on these plan forms and are mainly a question of compromise between high lift, low high-speed drag and structure weight. To discuss these aspects would be outside the scope of this lecture, but stability and control considerations do also have an important influence and will now be considered.

4.3.1. *Low-speed behaviour*

(a) The Stall

From the earliest days of flying the stall has been perhaps the major hazard in the fixed wing aeroplane. It cannot be eliminated but at least it can be tamed. To do this it is necessary to:—

(*a*) Avoid large rolling and pitching displacements.
(*b*) Provide adequate control.
(*c*) Provide warning of the approach of the stall.

In the past lateral instability with wing dropping has always been the main problem. Swept-back wings tend to have a tip stall, which, if the wing is also of high aspect ratio, can cause a nose-up pitch and pronounced longitudinal instability, which can then be more serious

than the wing dropping tendency. In the thin swept-back wing there is one of the most severe clashes of requirements, as the better their high-speed performance the greater the difficulty at low speeds.

Consider the simplest stall in which the whole wing stalls at once. Every section will give of its maximum just before the stall and therefore the overall $C_{L\max}$ will be the highest possible. However, such a wing will possess inherent lateral instability; owing to the negative lift slope just past the stall any slight initial rolling tendency will cause the lift on the diverging wing to drop even more, giving a violent divergence in roll. This basic tendency to diverge in roll is present on all wings at the stall, and the fundamental way of lessening it is to prevent all the wing from stalling at the same time and instead, to have the stall develop progressively. Thus what is termed the stall is in fact only a partial stall, with sufficient of the wing remaining unstalled to preserve at least a modicum of damping in roll. A stall starting somewhere over the inner half of the wing and developing gradually to cover that area seems to give the best behaviour, the great aim being to prevent the tips from stalling suddenly.

In causing part of the wing to stall first there will be a loss of overall $C_{L\max}$ and a corresponding penalty in take-off and landing performance, so the aim in design is to strike a careful balance between these two requirements.

The real difficulty comes in that however much one may theorise on the make-up of a good or bad stall it is still not easy to design the required qualities into the aeroplane. The difficulty arises primarily because theory is limited in dealing with the complex problems of the boundary layer and flow separation, and because the obvious stand-by of experimental methods, *i.e.* the wind tunnel, is seriously handicapped by the low Reynolds numbers. Thus the designer must do the best he can by experience, model test and theory, but has to be prepared for a considerable amount of expensive cut and try modifications in the prototype flight testing stage.

There are a large number of parameters which affect the stall: wing section, wing sweep, aspect ratio and taper ratio, Reynolds number, Mach number, roughness and flaps; and there are an equally large number of devices available to deal with it—slats, nose flaps, drooped noses, fences, boundary layer control, and so on. This means that although the problem is complicated there may be a variety of solutions, as is obvious in comparing the crescent wing with the delta wing. The crescent wing aims at preventing a tip stall in the usual way, but the delta wing appears at first sight to contradict all normal principles of stall control as the stall starts right at the tip. It is acceptable, presumably because only a small area of tip is involved and the spread of the stall inboard is gradual. Also, the delta wing has a low aspect ratio and it is well established that the tip stalling causes hardly any longitudinal instability on a swept-back wing, provided that the aspect ratio is low enough.

The Comet uses two forms of spanwise stall control, a built-in increase of nose radius towards the tip, the primary control, and the secondary control, developed by model and flight test of the leading edge fence. It has been found that the chordwise extent of the fence may be adjusted to suit the sections' stalling characteristics, *i.e.* whether a leading edge or trailing edge stall. In our experience fences have been a most effective and flexible device for adjusting the stalling behaviour in the prototype flight testing stage. On the D.H.108 tip slats were effective but full span slats brought a fairly severe wing drop, which, however, could then be cured by fitting two fences. On the Boeing B-47 the pod engines are reported as preventing the tip stall, presumably because they reduce the local incidence of the air flow above them.

To conclude; it can be seen that to design for good stalling qualities is a problem which it is not too easy to formulate and still more difficult to solve. Any research work aimed at a better understanding of the many variables is of great value, but it should be remembered that it is necessary also to be able to interpret model test results in terms of full scale behaviour before the designer can have confidence in the results. Thus the research worker and the designer should work closely together in this problem so as to be able to interpret each others findings.

(*b*) *Low-speed lateral control*

If the tip stall has been prevented the ailerons may still be reasonably effective although less powerful than on a straight wing. Thus the Comet, D.H. 110, Valiant and B-47 all have conventional ailerons; the crescent wing has them also but is helped by the reduced sweep-back at the tips, while on the delta, although the tips stall early, the conventional aileron still seems to be adequate—possibly because the aileron itself is straight across the air flow and not swept.

There are two further possibilities—firstly to use a mid-span aileron and thus avoid the bad tip region altogether, and secondly to prevent adverse yaw.

At high speeds the rate of roll may be calculated with sufficient accuracy by assuming pure rolling motion only. At low speeds the associated yawing response is very important. It arises from three causes:—

(*i*) The adverse yawing moment produced by aileron deflection (n_ξ).

(*ii*) The adverse yawing moment caused by the response in roll (n_p).

(*iii*) The adverse side slip induced by the angle of bank (ϕ).

These three terms all add up and produce a resultant sideslip

$$\beta = \frac{f(n_\xi, n_p, \phi)}{n_v}$$

and a corresponding adverse rolling moment

$$= \frac{f(n_\xi, n_p, \phi) \times l_v}{n_v}.$$

n_ξ, n_p and ϕ all increase with C_L, while on a swept-back wing l_v also increases rapidly with C_L, so that the effect is worst at low speeds when the aileron is most needed. Fig. 11 shows the effect of adverse yaw on the steady rate of roll.

As the response to aileron on the approach often determines the aileron size, so it would be very desirable to eliminate the adverse yaw. This could be done by linking the rudder in with the ailerons so that sufficient rudder is put on to counter the adverse yawing moment. This should enable a reduction in aileron size or improvement in aileron control to be obtained. Spoilers also have been suggested to help this problem.

4.3.2. *Wing behaviour at high speed*

Ten years ago the speed of sound represented a major obstacle. Serious increases in drag and major stability and control difficulties existed; thus violent trim changes, instabilities, buffeting, and complete loss of control effectiveness appeared to render further progress very hazardous. Now the sound "barrier" has been pierced many times and instead we talk of *transonic* speeds. How has this been achieved?—by four means:—

1. Thin wings and tails
2. Sweepback
3. Low aspect ratio
4. Full chord all-moving controls.

These features are combined in varying ways depending on the role of the aircraft, *e.g.* long range aircraft may have high aspect ratio, while the transonic and supersonic fighter will certainly have low aspect ratio and it may be observed that the delta plan form combines three of the foregoing four features.

However, many problems still remain, some of which are mentioned below.

(*a*) *High lift*

As at low speeds the swept-back wing is liable to tip stalling troubles at high M. The methods of curing it will, however, differ—the emphasis shifting from the leading edge to mid chord owing to the presence of shock waves. Thus, improved flow is most likely to be effected by making the wing thinner, but other devices designed to re-energise or direct the boundary layer are possible, namely, vortex generators ahead of the maximum thickness to re-energise the boundary layer and fences to direct it.

(*b*) *Transonic trim changes*

Owing to the fundamental changes of flow at supersonic speeds it will probably be necessary to accept an eventual nose down trim change in the transition from high subsonic to supersonic speeds, but it would be a great asset if the nose down trim change could at least be postponed to beyond the cruising speed range. The maximum cruising speed is not likely to be much above the drag divergence Mach number and, therefore, this should not prove an impossible aim. Analysis of existing data shows that some aircraft do in fact have a small nose-up trim change at about the critical M which is an ideal characteristic, giving an automatic recovery from any inadvertent speed rise. On the other hand some aircraft go progressively nose down and start doing so at Mach numbers which may be very near, or at, the maximum cruising speed. It appears that the

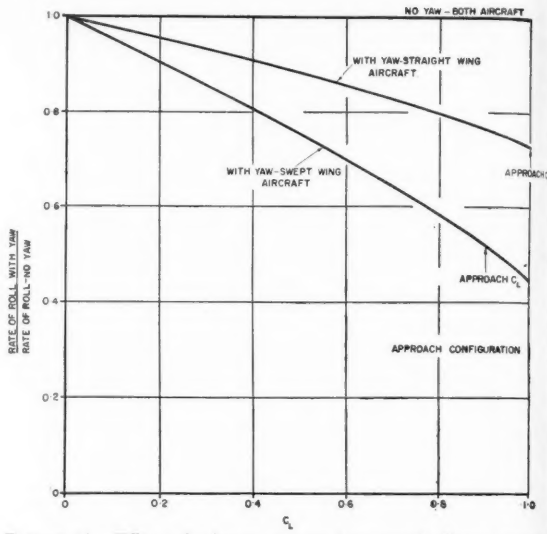


FIGURE 11. Effect of adverse yaw on low speed aileron control.

latter is the more fundamental behaviour and more likely to occur on a clean aircraft, whereas aircraft with nacelles, large bodies in extreme high or low wing positions, may have a temporary kink induced in the $C_M - M$ curve, *see* Fig. 12.

There appears to be no method at present for designing such a kink into the $C_M - M$ curve. Calculations indicate that to cause a suitable kink would not require a very large change in pressure distribution, and if a relatively small temporary increase in lift could be produced in the region of the wing centre section it would produce a nose-up moment from both its direct moment about the c.g. and from the associated increase of downwash at the tail. As it is most probable that the initial nose down C_M originates at the centre section some small deliberate modification of section shape and camber here might prove effective.

Some evidence on the Vampire suggests that the trim change is very sensitive to the local flow around the fuselage and wing roots, small changes in cowling shape and repositioning of some small external air intakes changing the trim change from moderate nose up to pronounced nose down.

Failing an inherent solution the aircraft can either be fitted with an automatic dive recovery device, or with auto-stabilisation.

(*c*) *Lateral control at high speed*

One of the biggest control problems today concerns the form of control to use at transonic and supersonic speeds. Theory indicates that at supersonic speeds the flap type control becomes relatively ineffective as it can only produce lift on its own surface and cannot induce lift on the main surface ahead, as at subsonic speeds.

Thus there is a strong incentive to increase the control chord ratio, of which the all moving tip is the ultimate form. A more serious problem is the even larger loss of control at transonic speeds but there is hope that on the thin sections, which will be used anyway, this latter loss will be avoided, and also the

related problem of transonic wing dropping. The mid-span aileron is another possible solution, as by avoiding the cross flow near the wing tips it is relatively more effective at transonic speeds.

Aeroelastic considerations are also very important and are discussed in the following section.

(*d*) *Aeroelastic effects in wing design*

Inadequate wing stiffness may lead to the following forms of unsatisfactory flight behaviour.

(*i*) Wing flutter.
(*ii*) Loss of aileron control (aileron reversal).
(*iii*) Wing divergence.
(*iv*) Loss of longitudinal stability.

To avoid these troubles the prime requirement is for a certain torsional stiffness, but on swept wings flexural stiffness plays a part as well.

On a straight wing aileron reversal speed depends directly upon the torsional stiffness, while on the swept-back wing the flexural flexibility has an additional adverse effect. The requirements are for the aileron reversal speed to be at least 15 per cent. above the dive speed, and to meet this on a straight wing usually means rather more stiffness than for flutter prevention. On the swept-back wing the flutter stiffness required tends to decrease, but the aileron reversal requirements become relatively more difficult to meet.

Spoilers appear to be one obvious answer to this problem as they reduce greatly the twisting moment applied to the wing. There is little experience of the use of spoilers in this country but they are being used with apparent success on several American aeroplanes, *e.g.* B-52. There have always been two doubts about the use of spoilers; the time lag in response, and the buffeting they may cause on the wing, particularly on any light flap structures behind them, and any tail buffeting in a roll. Both these doubts are real but it would seem to be well worth while making a full scale experiment on their use, as substantial dividends in reduced wing structure weight may follow.

Another solution often suggested is to use an all-moving tip control. This also sounds attractive for supersonic speeds owing to its inherently greater efficiency there than the trailing edge flap control. However, to make this control perform all the functions of an aileron it must work at low approach speeds and to deflect the whole tip may then cause it to stall. Further, it seems that the size of tip required at low speeds would be considerable, of the order of 30-35 per cent. span and its operation would present considerable practical difficulties.

However, a compromise solution of having a diagonal hinge line appears attractive—a small area of all-moving tip tapering off rapidly to a small chord aileron inboard.

Wing divergence is not normally a real problem at all as the divergence speed is usually higher than the flutter and aileron reversal speeds.

With increasing lift the swept-back wing will deflect in flexture and torsion. The bending deflection, because of the sweepback, will cause a reduction in the line of flight incidence at the tip and will be destabilising, while the torsional deflection will normally be nose up, *i.e.* stabilising. This stability problem is usually most serious on high aspect ratio swept wings, when the bending effect is the larger and there can be a resultant loss in manoeuvre stability of $0{\cdot}05\bar{c}$ or even $0{\cdot}10\bar{c}$ at high speeds. The "aero-isoclinic" wing has been suggested by Prof. G. T. R. Hill[4] as a means of defeating this effect, in principle the idea being to increase the torsional deflection to offset that due to flexure. This could be done either by moving the flexural axis aft (a difficult task with orthodox structures) or by reducing the torsional stiffness. Aileron reversal could then be looked after either by an all-moving tip or spoilers. The main doubt is whether flutter will still be satisfactory. A similar object is achieved in a simpler way by having less sweepback on the outer wing, as in the cranked or crescent wing. The outer wing then tends to have a far back "flexural axis" and, therefore, the torque will cause larger nose up twists at the tip to compensate those due to bending.

Generally speaking wing aerolastic effects assume a major importance in the structural and aerodynamic design of the wing. On many aircraft the outer wing is designed by torsional stiffness and there is therefore a desire to improve methods of dealing with the problem. Modern high-speed computing machinery will greatly facilitate design investigations, but the biggest doubt remains on the aerodynamic derivatives, particularly for the critical design conditions at high subsonic speeds. The need is primarily for the spanwise and chordwise distributions of the air forces, both due to incidence and control deflections. These can only be obtained by pressure plotting, and such tests in a wind tunnel, although lengthy and tedious to reduce, are literally "worth their weight in gold" to the aeroelastic specialist.

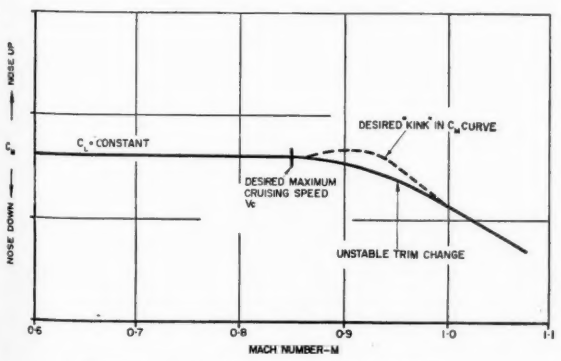


FIGURE 12. Mach number trim change.

(*e*) *Conclusion*

The varying wing plan forms represent attempts to reconcile the requirements of high- and low-speed flight; it can be seen that there are a surprising number of solutions and without considering other aspects of the whole design it is not possible to say which, if any, is the best—security requirements will in any case preclude any early disclosure of the relevant facts and figures. It is apparent however that sweepback brings

many difficulties with it, and the thin straight wing of low aspect ratio does offer another solution worth considering, particularly for higher supersonic speeds.

4.4. SOME MISCELLANEOUS DESIGN PROBLEMS

4.4.1. *Power boost versus spring tabs*

It is necessary to consider these for two reasons:—

1. Due to large size of aircraft.
2. Due to high speeds.

Factors affecting the choice between power boost and spring tabs are:—

(*a*) *Control effectiveness*

The spring or servo tab is an automatic aerodynamic balance, which relies on reasonable aerodynamic properties from the elevator and tab; at large control deflections ($> 15°$) elevator hinge moments tend to increase, and tab hinge moments to decrease, so large control angles are not easy to use with a servo tab scheme, but can be used with a power control. Further, with a servo tab there is a considerable loss in overall control power due to the tab lift acting in the wrong direction. Thus in the critical low speed range the servo tab will need larger controls than the power boost design. Increase in control size must be paid for by reductions either in flap span or the torsion box area of the main supporting structure and by increases in mass balance weights.

At high Mach numbers there is at present little doubt that the power control is essential owing to the extremely nebulous characteristics of tabs at transonic speeds.

(*b*) *Flutter considerations*

On common sense grounds the spring or servo tab looks a difficult object to get right and avoid any flutter, buzz or vibration troubles. While much has been learnt about the flutter problem there is still a considerable gamble in designing a new system to operate at quite moderate speeds, and the designer must be prepared to face a period of flight development to eliminate any troubles. Again, at high Mach numbers the difficulty is much greater and to the flutter problem are added those due to flow separation. Irreversible power controls with the booster as close as possible to the surface are a great asset in holding the control fixed at transonic speeds, and so minimising the likelihood of any buzz or vibration. They also help the basic flutter problem.

One rather elusive attraction of fully powered, *i.e.* irreversible, controls is that they lead to the possibility of doing away with mass balance weights. The aim is then to prevent control surface flutter by providing adequate stiffness, but it has been found that the ordinary mounting of a booster does not necessarily give anything like the stiffness required for flutter prevention. Also, moderate increases in circuit stiffness often tend to postpone the flutter speed rather than eliminate it (mass balance tends more to eliminate flutter). Thus considerable reliance has to be placed on the accuracy of the flutter calculations which indicate how much stiffness is required. In any case considerable effort must be put in to making sure that the complete assembly of control, booster unit and booster mounting structure is really stiff enough. Thus at present the practice is usually to provide mass balance weights even on a fully powered control, but it is likely that they will be gradually reduced and finally removed in the future.

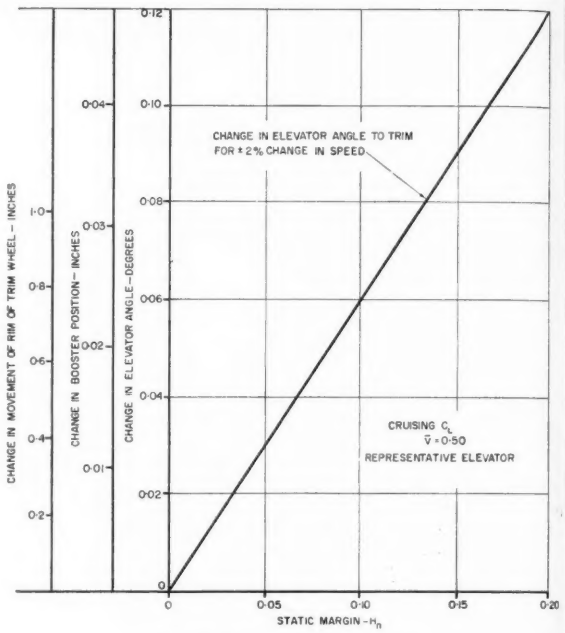

CHANGE IN MOVEMENT OF RIM OF TRIM WHEEL – INCHES
CHANGE IN BOOSTER POSITION – INCHES
CHANGE IN ELEVATOR ANGLE – DEGREES
CHANGE IN ELEVATOR ANGLE TO TRIM FOR ±2% CHANGE IN SPEED
CRUISING C_L $\bar{V}$ = 0·50
REPRESENTATIVE ELEVATOR
STATIC MARGIN – H_n

FIGURE 13. Sensitivity of trim.

(*c*) *Feel*

Feel requirements have been discussed in Section 2.3.1. Both spring tabs and power boost have their own problems, spring tabs partly disguise from the pilot the loads he is putting into the aeroplane, but, theoretically at least, can be made to give something like the right feel variation with speed by manipulating the tabs' own hinge moments.

Fully powered controls, with no feedback of the aerodynamic load, do land us fair and square in the feel problem.

On the Comet a policy has been followed of making the feel as simple as possible and just a simple non-linear spring feel has been used, it being considered best to add more complicated feel devices only if they were shown to be really necessary. There are many proposals for such devices; "q" feel, V feel, M feel, variable circuit damping, variable circuit gearing, inertia weights and "g" restrictors. The wide variation in the feel characteristics of these various devices is shown by a few qualitative lines on the feel diagram (Fig. 1). At subsonic speeds the variable gear is seen to look promising. One thing is reasonably certain, the need to get low friction forces and break out forces to a minimum especially on the elevator, but beyond this it is not certain that other complications are really essential.

As mentioned in Section 2.3.1 power controls may introduce considerable difficulty in trimming exactly to a required speed.

Figure 13 shows the permissible tolerance in elevator angle to trim within ± 2 per cent. of a required speed —a reasonable standard to demand. It is seen that with a static margin of $0{\cdot}05\bar{c}$ the control must be selected to an accuracy of about $\pm 0{\cdot}03°$. With a normal gearing this is equivalent to about ± 0·012 in. at the booster unit. It is immediately apparent that this is going to be difficult to achieve, and calls for close attention to the detailed design of the values, and to items such as friction, backlash, and so on.

(*d*) *Mechanical problems*

On the Comet it was decided early on to go for a fully powered control to avoid the acute difficulties of producing a large closely balanced aerodynamic control operating at high Mach numbers. It was also soon decided that manual reversion would be difficult from a power-assisted control and very difficult from an irreversible control, and it was therefore decided to go in for the necessary duplication of the primary system to ensure safety in the event of any reasonable combination of failures.

Experience has given convincing proof of the value of power controls on the Comet—on the prototype 150 hours of flying were completed in the first 150 days, with very little effort being required to get the controls aerodynamically right, and in the 20,000 hours of operational service which have now been flown the power controls have behaved quite satisfactorily: the inevitable teething troubles have not been of a fundamental character and have mostly been rectified in the normal course of development.

(*e*) *Conclusion*

Once the mechanical problems of a power control are faced it appears that on balance they are preferable to spring or servo tabs, the main advantages being the smaller control sizes, the greatly reduced flutter problem and the relative ease of development in the prototype and early production period.

4.4.2. *Repeatability*

We are familiar with the old problem of making the control forces repeatable on production aircraft with the difficulty increasing rapidly as the control was more closely balanced. However, there are now other flight characteristics which may vary widely from one aircraft to another such as

(*a*) Low-speed stalling qualities.
(*b*) High-speed stalling qualities.
(*c*) Transonic trim changes.
(*d*) Transonic buffeting.

This is a formidable list and if all these qualities differ on production aeroplanes of how much value are the prototype flight tests? Obviously one must try to make all of the type behave roughly alike, and the problem must be tackled along both of two lines:—

(*i*) By improving standards of manufacturing accuracy. This is easy to say but more difficult to achieve. In the past many imperfections have been "got away with," such as badly-fitting control shrouds, gaps at hinges, badly fitting cowlings, thin wavy panels, and so on. Much can be done by careful attention to detail design and by laying down tolerances on many more items than has been done in the past, but it remains difficult to say just what numerical values those tolerances should have. Commonsense and judgment seem the only practical ways of deciding on these tolerances. Much can be done once the need is fully realised by production engineers.

(*ii*) By designing an aeroplane which is intrinsically less sensitive to small differences in profile. This must remain the best cure but it is not always certain what arrangements will produce the desired insensitivity, and in some cases it is difficult to avoid a sensitive arrangement because of conflicting requirements, *e.g.* thin wings are good at high Mach numbers but can be touchy at low speeds. Little more can be said at present, other than that the existence of the problem should be more widely realised and taken into account in aerodynamic research, *e.g.* the full-scale aeroplane can never hope to be as perfect aerodynamically as the smooth highly-polished wind tunnel model.

5. Conclusions

It is suggested that the outstanding problems of stability and control which the designer faces today are:—

1. It is still not possible to formulate all the standards of stability and control which correspond to the pilots' judgment of good handling qualities. The existence of this deficiency in knowledge has been accentuated recently by the need to decide what to design into artificial feel systems, but the problem is wider than this and extends particularly to the make up of good dynamic stability and good dynamic response to control application.

2. The need for more knowledge of the air forces has extended beyond the transonic into the supersonic range. More information on oscillatory derivatives is an urgent necessity.

3. Flow separation is at the root of many troubles, particularly the low-speed stall, the high-speed stall and buffeting; and it influences many others such as transonic trim changes, losses of damping and control effectiveness, and the considerable repeatability problem. Any fundamental work on the mechanics of the boundary layer is therefore very important and one looks hopefully at boundary layer control as a possible solution of the future.

4. Automatic stabilisation is here to stay. It should not be regarded as something to be used as a last resort, for if designed into the aeroplane from the start it could lead to a better overall performance. Thus the aim would be to design towards a lower standard acceptable when the auto-stabiliser has failed, while having really good handling qualities when it is working.

5. It is not easy to decide where to put the tail on a high-speed aircraft, and it would be valuable to know whether it is essential for transonic damping on a supersonic aeroplane.

6. Power boost is an established and reliable method of operating controls. The main problems centre on the detail mechanical design of the control circuit and booster value, and on deciding what form the artificial feel should take.

7. There are several sorts of lateral control possible on high speed plan forms, comprising the conventional aileron, the all-moving tip in partial or complete form, spoilers, and mid-span ailerons. As a counterpart to the all-moving tail developments it would be particularly interesting to see some high speed flight tests on an all-moving tip and some full-scale tests of spoilers on a high speed aircraft would also be very valuable.

8. Plan forms suitable for high subsonic flight vary considerably. For supersonic speeds there is no doubt as to the need for thinness and low aspect ratio; but the stability difficulties associated with high sweepback mean that on designs for higher supersonic speeds the straight wing again merits consideration.

ACKNOWLEDGMENT

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Discussion

HANDEL DAVIES (A. & A.E.E., Boscombe Down, *Fellow*): Fig. 1 in Mr. Wimpenny's paper was extremely interesting, but did he not think that the size and operational role of the aircraft were not important factors in the feel requirements for an aircraft? If they had a very small, highly manoeuvrable fighter aircraft, which had to be used among other things to aim guns, and also had a very wide speed range, it was fairly clear that a simple, unsophisticated feel system, such as fixed springs, would not be good enough. At high speed, the aircraft would be so sensitive that control, particularly during aiming flight, would become difficult. For a large transport aircraft a simpler and less sophisticated type of feel system might be feasible, since such an aircraft was much less sensitive, having longer response times. It seemed to him that feel requirements were very much a function of the type of aircraft and its role.

Also on Fig. 1, he wondered whether sometimes in this question of feel with power control they did not allow themselves to be influenced a little unduly, when talking about the *pilot's* requirements, by the mechanical limitations which applied in practice. Mr. Wimpenny's diagram, for example, indicated that a control system in which there was no movement—nothing but force—would be a desirable one but would be far from best. Some basic research work on the human operator, however, particularly by Gibbs at Cambridge, had indicated that the ideal form of control to provide for a human operator was one in which there was no movement at all but in which the whole control depended simply on force variation.

Therefore, when discussing what the *pilot* wanted, ought they to admit that the best thing of all was a control in which there was no movement, but that this was impracticable at present for engineering reasons.

He endorsed Mr. Wimpenny's remarks about the need to keep friction backlash and flexibility in the control circuit down. He could not understand why—in spite of the obvious need for such characteristics and in spite of the fact that many years ago there were aircraft flying with manual controls (controls which had to have a large amount of force transmitted through them) and it was possible then to design these controls with practically zero friction, no backlash and very little flexibility—in this Coronation year of 1953 they had aircraft flying with power controls which had the most appalling break-out forces in the control circuit.

In fairness, he must confess that although these break-out forces had appalled him, in practically every case where those aircraft had been flown by pilots, whether airline pilots, test pilots or Royal Air Force pilots, they had all come back and said "This is not too bad. You are being far too pessimistic." What the explanation of that was, he did not know, but he sometimes thought it might be analogous to the case of the tight-rope walker. If a tight rope were stretched across this room and someone in the audience was asked to walk across it, he would probably find difficulty in doing so; but a professional tight-rope walker who had been doing it for 3,000 hours, could do it quite easily. But if every member of the audience then threw footballs at the professional tight-rope walker, he too would find it difficult. He wondered whether the highly experienced pilot flying these aircraft in very bad conditions, would be in the position of the professional tight-rope walker having the footballs thrown at him.

He agreed with Mr. Wimpenny's view that they ought to make use of auto-stabilisation, with one proviso: that they would have to be pretty careful about how far they could go in this matter of relaxing requirements. It was obvious that there were some relaxations they could not make. For instance, they could not accept an aircraft which would, without auto-stabilisation, have negative longitudinal damping in the transonic range; but where the line would be drawn beyond that it would be quite difficult to determine. Would Mr. Wimpenny consider that an aircraft with a divergent Dutch roll oscillation on the approach would be acceptable or not if it had an auto-stabiliser?

There were some misconceptions on repeatability. Many of them, when talking to people in the Industry particularly concerned with production, had been scoffed at and told "You are just a lot of long-haired scientists. You must realise the practical limitations here." It was true that they were long-haired scientists, and sometimes they had asked for a finish on a wing surface accurate to one-thousandth of an inch, which it was clearly difficult to produce; but the sort of lack of repeatability which they were often getting on aircraft at the present time was not in that category at all. For example, in the case of one aircraft which they tested recently at Boscombe Down, they found that the production aircraft had unexpected characteristics in the high speed range. When they examined it they found that the wing surface had a very marked and very obvious discontinuity in it, and at one point there was an inaccuracy of the order of one quarter of an inch in the wing profile. They could not hope to achieve well-behaved transonic aircraft in production if the Industry could not do better than this.

G. H. LEE (Handley-Page Ltd., *Fellow*): He agreed very largely with the general tone of Mr. Wimpenny's paper. It seemed that the thinking they did at de Havilland's was very similar on those problems to the thinking they tried to do at Handley-Page. That was encouraging.

Another important point was that their recent experience agreed with quite a lot of the actual numbers given in the paper. That was probably very significant. It might mean that they were beginning to get things on a quantitative basis, which was the only real basis on which one could really design and predict things and avoid the trial and error processes which were so expensive. The fact that de Havilland's numbers seemed to agree with theirs might therefore mean quite a lot.

He agreed on the question of irreversible power controls and also the way Mr. Wimpenny had explained some of the undoubted virtues of the crescent plan form!

One could sometimes do a lot to improve a wing or tail unit aeroelastically by making it flexible if that could be done in the right sort of way; it was mentioned in the paper, and it was very important. They had used that principle on the Victor, particularly on the tail, and he thought they had got a lot of advantage from doing that. The moral seemed to be that one should not rely too much on meeting a stiffness criterion. If they made everything very stiff, they might not have such a good aeroplane as if, carefully and intelligently, they made it flexible in the right places.

Mr. Wimpenny suggested inter-connecting aileron and rudders. He did not know whether it was generally known, but they had done that on the Victor. They intended ultimately that the inter-connecting gearing should vary with speed. At present it was adjusted by the pilot, and they wanted to find out what inter-connection he thought was best.

Mr. Wimpenny referred to the importance of flexural stiffness of the wing from the point of view of reversal. He did not agree with that—not now. He knew that once he said to the Society that wing flexibility was important in reversal on the swept-back wing, but he did not now think it was really very important after all. He would like Mr. Wimpenny's comment on that.

Imagine, to begin with, that the wing was rigid and that it was held fixed, and now suppose that an aileron was deflected downwards. Under these conditions the wing would experience a certain upwards aerodynamic loading which would be small over that part of the wing inboard of the aileron and large over that part of the wing that was covered by the aileron span.

In all actual cases, this loading was reacted by some combination of the following three types of loading:—

(*i*) The loading due to giving the aeroplane an angular acceleration, that was an inertia loading which built up from zero near the root to a maximum near the tip.

(*ii*) The loading due to the rolling velocity of the aeroplane, which was also small at the root and high near the tip.

(*iii*) The loading due to the wing twist, which again was small at the root and had its peak near the tip.

If the loading due to aileron were positive, then equilibrium was obtained by adding some mixture of the above three negative loadings. It would be realised that all of the four loadings in question were of somewhat similar shape, that was they were low at the root and high at the tip and so when they were integrated twice spanwise to get the resultant bending moment, he thought it would be found in nearly all cases that the resultant bending moments were quite small; it would be remembered that the bending moment and shear were zero at the tip and the shear was zero at the root.

For those reasons he did not think that wing bending stiffness played a major part in the problem of aileron reversal.

There was a possible exception that if an aileron were applied suddenly they might get oscillations set up in the wing that were not covered by his argument and this might give a short duration effect, which was different from the kind of thing considered above.

PROFESSOR A. R. COLLAR (University of Bristol, *Fellow*): What Mr. Wimpenny had dealt with was, he supposed, a good half of aeronautical engineering.

Mr. Wimpenny said that the stiffness criteria for

tailplanes, elevators and fuselages were introduced in 1943. He was rather grateful that Mr. Wimpenny did not give a reference here, since it would have been a very personal one so far as he was concerned. He said this because he rather agreed with what Mr. Wimpenny said.

In 1943 the question of tail flexibility was becoming important and was really brought urgently to their notice by gliders, a very special category of aircraft which they did not use much nowadays. Unusual troubles which occurred with that type of aircraft led them to look at tail flexibilities: fuselages, tailplanes, elevators, and all the rest. It showed that there was a very great deal that was not understood and appreciated about that subject, and they felt that the first thing to do was to call for design criteria laying down minimum standards of stiffness for each of the components. While that was a safe thing to do in an emergency, it was not necessarily the best thing (especially for permanent use), and he would be the last to maintain that it was. As Mr. Wimpenny and Mr. Lee had said, there was much to be said for dealing with the problem sensibly and intelligently, rather than simply according to the book.

He underlined what Mr. Wimpenny said in Section 4.2.1.6, because he felt it needed emphasising: "It is a good practice to work out each case fully on its own merits." He supported Mr. Wimpenny and Mr. Lee in this matter, but only provided that each case was looked at carefully on its own merits.

Turning to the question of aeroelasticity effects on wing design—the "long and sombre list" to which Mr. Wimpenny referred—he wanted only to mention the aileron reversal question and, as Mr. Lee did, the question of bending stiffness. Bending stiffness in this connection had been very much of a red herring. If one thought of the problem in terms of the required stiffness at a section *containing the wind direction*, that was strictly all that was necessary. This contention was borne out quite fully by some experiments they had done recently at the University of Bristol. It meant that the intrinsic torsional stiffness of the wing must be put up a little bit, because it was dealing both with structural bending and torsion; but the situation was clarified a great deal if one thought of the torsional stiffness only taken in a section containing the wind direction. Then, for a given "streamwise" torsional stiffness, the reversal speed rose with increasing sweepback.

A point he was very pleased Mr. Wimpenny referred to was that in which he suggested that the compromise solution of having a diagonal hinge line for one's aileron or elevator appeared to be attractive. He had thought that to be attractive for a very long time but was under the impression that no one else thought so.

Mr. Wimpenny had stressed the necessity for measuring aerodynamic loads by pressure plotting, both for changes of incidence and for control deflection. He suggested that in addition one needed to measure the loading on distorted wings—not distorted in the sectional sense, but bent and twisted in the manner produced by aerodynamic loads. In the University of Bristol, Dr. Black had experimented with a wing of Perspex, which had the useful property that when it was boiled in water it became plastic and could be moulded to any desired shape and then allowed to set. In its distorted form it could then be pressure-plotted. The material had the additional property that—perhaps because it had a long "memory" deriving from its long molecular chain—it reverted to its undistorted shape when re-boiled in the absence of stress, and one could do a check test. It was a convenient method of testing distorted profiles, and well worth following up.

J. M. HARDMAN (Armstrong Whitworth Aircraft Co. Ltd., *Graduate*): The curve shown in Fig. 4 of percentage change in speed due to small c.g. changes, was presumably over-simplified in neglecting the rapid changes of drag and aerodynamic centre position near the critical Mach number. In addition to the modifications introduced by these two parameters, the effect of aircraft inertia was surely all-important, since the initial response to a given increment of pitching moment was independent of the static stability and was proportional to inertia. This effect was obviously most noticeable at high altitudes and for high wing loadings. It was largely because of this that flight at transonic speeds, when static stability must become negative, was possible, the lag in response due to inertia giving the pilot time to react.

Aircraft inertia also had an important bearing upon dynamic lateral stability characteristics. For example, low moment of inertia in roll relative to that in yaw could make the position of the oscillatory stability boundary almost independent of the aerodynamic derivatives, although for an aircraft with this type of inertia distribution it was important to ensure that the principal axis of roll was not inclined nose down relative to the line of flight.

It was interesting to note that although wing distortion resulted in a loss of manoeuvre stability in many cases, the reduction of wing lift curve slope that resulted from the same distortion meant that the contribution of the tailplane to longitudinal stability was increased. That was, of course, dependent upon the tail and rear fuselage stiffness being such that the effective lift curve slope of the tailplane did not decrease as rapidly as that of the wing. The matter was important since in an aircraft with fully power-operated controls the pilot only knew about the stick-fixed stability which was dependent upon the position of the aerodynamic centre of the aircraft with tail on.

H. F. VESSEY (Royal Aircraft Establishment, *Fellow*): Mr. Wimpenny mentioned the use of the Helix angle for defining the take-off rate of roll. He agreed that the Helix angle was not a very good method either for defining take-off or, probably, for defining the high-speed rate of roll. He thought that the actual requirements were now being reconsidered on the basis of a time to roll a specified number of degrees.

Mr. Wimpenny mentioned the desirable nose-up pitching moment at the beginning of transonic effects, and seemed to be asking for a formula. He could not give the formula, but there were a number of aircraft which did pitch up before the final pitch-down, which he thought was almost universal. In fact, they could

have nose down, nose up, and then final nose down; or nose up, nose down, nose up again, and then nose down. As regards the quantities which contributed to this, Mr. Wimpenny rather over-simplified the case as the movement of the aerodynamic centre and the downwash.

Those were probably the major factors, and except for a tailless aircraft the down-wash was generally the most important, but there was also the change of C_{M_0} and the slope of the lift curve, both on the wings and on the tail; and, further, there were aeroelastic effects. The final behaviour was the difference of, in the first case, two large quantities with a number of smaller quantities thrown in, and he thought it was true to say that it would probably be impossible to predict to the degree of accuracy required to give a gentle nose-up pitching moment to provide this stabilising effect on cruising. Probably they wanted an independent invention which would do it for them.

When Mr. Wimpenny talked about pilots he thought he was a little unkind. Far from being a poor servo-mechanism, pilots were very much better than the best servo-mechanism that they had managed to produce so far. They were so much more adaptable. They were not good servo-mechanisms in the sense that they could react as rapidly and perhaps even do quite the predicted things, but the pilot was a very much more adaptable mechanism and quite a lot of aeroplanes would never have been flown had they not had adaptable servo-mechanisms of this type.

F. W. MEREDITH (Smith's Instruments, *Fellow*): On the question of "break-out" forces, he had always been surprised that some of the displacement-force diagrams he had seen were acceptable to a pilot, but he was assured that a large number of pilots had found them acceptable. He wondered whether the reason was that, for delicate flying, they did not break-out at all: that they did all the flying through the trim and only used the break-out for gross movements. As a designer of an automatic pilot, he was not in the fortunate position of being able to avoid making delicate movements through the normal controls.

On the question of automatic yaw damping and the two standards, he heartily agreed with Mr. Wimpenny, but would like to add a word of warning which another speaker had already sounded. One must be perfectly sure that the aircraft was still safe in the event of failure of the yaw damper, even in the hands of a pilot who had got out of practice in dealing with such an aeroplane; because when a pilot had come to trust the automatic stabiliser the skill required for dealing with the situation might be lost. He sounded this warning because he did not want the aerodynamic designers to go too far in relaxing on the second standard, as he might be responsible for some of the automatic dampers which would otherwise be expected to be infallible.

There were one or two points that perhaps Mr. Wimpenny might deal with in his written reply. For the benefit of people like himself who were not, perhaps, specialists in modern aerodynamics, he would like to know exactly what Mr. Wimpenny meant by the phrase "manoeuvre stability" in an early part of the paper. He said that manoeuvre stability increased as one approached the change of trim associated with the critical Mach number and that that alleviated the situation. Also, what was the difference between "buzz" and "flutter"?

In connection with the famous Fig. 1 which fascinated him, he assumed that the *J-K* curve was broadly the scheme that had been proposed by Mr. North, of Boulton Paul, in which there was a constant spring on the stick and the gearing between the stick and the power control was varied as a function of Q. It looked very pleasant on the diagram. The characteristic point sat most of the time in the middle of the "ideal region" and went out only for transonic speeds. He wondered whether it would not be possible to add to the control of the gear changing mechanism a further term which depended on the transonic condition and which would bring even that point to the middle again?

DR. K. DOETSCH (Royal Aircraft Establishment): Fig. 2 on the short period oscillation, appeared to him to be rather optimistic. The poorest damping, labelled "considerable oscillation," still showed a damping of the order which, in the lateral motion, was the optimum that they could achieve at present with the most effective practicable yaw-dampers. If the picture were extended to, say, one cycle to damp to half amplitude—which was the present-day requirement of AP.970 for the lateral case—the manoeuvre margin for the worst case quoted (the tailless aircraft at 40,000 ft.) would move right out of the picture to 18·5 per cent. In fact, however, the longitudinal damping of some present-day aircraft, even with tail, had already deteriorated to one cycle to half amplitude with quite normal manoeuvre margins.

To compensate for this rather pessimistic outlook, he wished to draw attention to another side of the picture which Mr. Wimpenny had not mentioned. Increasing inertia in pitch, particularly on delta aircraft, was beginning to add to the overall damping of the short period motion by lengthening the period, and thus permitting the plunging motion of the c.g. to develop properly. This in turn gave a phase lead to the large static stability moment, M_w, with respect to the phase of the pitch attitude, θ, and accounted already for half the total damping on some present day designs, tending to become more important. Moreover, this effect was not subject to much deterioration at high Mach numbers because it depended solely on the lift slope.

Mr. Wimpenny said he thought it possible to provide sufficient aerodynamic stability by aerodynamic means, but he did not agree. On page 340 he said that to increase the fin size could be helpful. That would not always be so, because increasing fin size also increased the frequency and, particularly for short-tailed aircraft, the effects on i_e and l_v might cancel out any advantage otherwise gained, because the addition was usually at the top end of the tail. It was a safe means, however, of improving the oscillatory stability if the basic n_v were very low.

The dihedral might be chosen for spiral stability reasons on straight-wing aircraft, but for swept wings

that could hardly be true. In most cases of swept-wing aircraft it would be desirable to reduce the basic l_v—that was, l_v at no lift—to zero or very near zero, if necessary by anhedral, to ensure stability of the lateral oscillation at higher lift coefficients.

Spiral stability, which had been stressed in the paper, appeared to him, for the time being at least, to have lost its significance, because, for one thing, with most power controls a trim deficiency caused a kind of lateral divergence which completely masked spiral stability or instability.

Regarding Fig. 8, he found it a pity that dynamic lateral stability should have been dealt with by means of the $n_v - l_v$ diagram when they now had methods of making full dynamic stability calculations with less effort. The diagram had to be drawn for all the different C_L values, products of inertia and the other parameters which were nowadays so important. It must also be remembered that if the stability boundaries only were drawn, there might even be doubt as to the direction of the change of n_v required to achieve better relative damping of the oscillation. The diagrams given showed the boundary only. If, however, curves were drawn for constant relative damping, the slope changed its sign.

He would not recommend auto-stabilisation to overcome adverse trim changes with Mach number. The function of the auto-stabiliser was to relieve the pilot of unnecessary strain, and not to make a dangerous aircraft safe. He thought Mr. Wimpenny would agree on that.

K. S. LAWSON (Vickers-Armstrongs, Weybridge): Mr. Wimpenny seemed to imply that one of their big difficulties was knowing what sort of feel to give the pilot, and that if they knew more about what he would like, it would be relatively easy to provide it.

He would like to take as an example the elevator feel problem on a hypothetical large aeroplane. They would consider that a c.g. range of 0·2 of the standard mean chord was required, and that the maximum weight of the aircraft would be twice the minimum. Assume further that they were going to design for a manoeuvre margin of 0·1 with the c.g. aft: there were several reasons for making this smaller, but a large value was favourable from the feel point of view.

With those assumptions, there was a range of 3:1 on manoeuvre margin and 2:1 on weight, so that with q feel there would be a 6:1 range on stick force per g due simply to c.g. and weight variation. In addition the effects of compressibility, distortion and height would have to be taken into account, and they would probably have an overall range of at least 10:1.

The main parameters were c.g. and weight, and only a relatively small improvement could be effected by relating the feel to E.A.S., Mach number, or height. It was difficult to see how a reasonable range of stick force per g could be obtained on such an aircraft with any combination of q feel, bob weights, variable gearings and so on, and he would like to know how Mr. Wimpenny proposed to get down to ranges of the order of 2 or 3 to 1, as shown in Fig. 1.

In the paper, Mr. Wimpenny suggested that rates of the order of 45 to 50 degrees per second might be needed on ailerons. They had made some response calculations for a large aircraft to find the effect of control rate of movement, and compared the results on the basis of time to reach a given angle of roll, pitch, or yaw. They considered rates up to 40 degrees a second, and found that there was very little improvement in response time due to increasing the control rate of movement above about 20 degrees per second.

He agreed wholeheartedly that a moving tail was very desirable on high speed aircraft, but not so much for the reasons given by Mr. Wimpenny. On a swept wing aircraft, it might be difficult to obtain a reasonably small change of trim due to lowering or raising flaps unless a moving tail was used. The reason was that the ΔC_{M_0} due to flap was considerably less than on an unswept wing, and with a normal tail volume coefficient there would probably be a large nose-up change of trim on lowering the flaps. With a moving tail, a smaller tail volume coefficient could be used, and the change of trim due to flaps reduced to an acceptable level.

Stability considerations constituted another strong case for the moving tailplane. In his opinion, the most efficient way of obtaining a given amount of stability was to keep the c.g. range as far forward as was practicable relative to the aerodynamic centre, so that the tail contribution to stability could be quite small. This would have the effect of minimising the reduction in stability due to compressibility and distortion. As an example, in the worst case the tail contribution to stability might be halved due to compressibility and distortion effects: thus with a low speed contribution of 0·3, the stability loss would be 0·15, whereas with a low speed contribution of 0·1, the loss would only be 0·05.

He did not altogether agree that the so-called flying tail was the best form of moving tail. He favoured the trimming tailplane with a conventional elevator, the main reason being that he could not see how the rate of movement that would be needed for landing—of the order of 10 degrees per second even with a geared elevator—could be reconciled with a rate which would be satisfactory at high speeds—probably about one quarter to one half a degree per second. How did Mr. Wimpenny propose to overcome this difficulty?

H. H. B. M. THOMAS (Royal Aircraft Establishment, *Assoc. Fellow*): He could not resist going back to an opening remark by Mr. Wimpenny when he said that the pilot initiated all the movements of the aircraft. He was sure pilots must often wish that they did!

He agreed with Mr. Davies and Mr. Meredith on the question of auto-stabilisation. He thought, for a slightly different reason, that there was a danger that in the kind of doctrine in which one said "Oh, well, we can let the aerodynamics go hang now; we have got something else to do the job for us," by opening the door to such a device the aerodynamics would certainly fly out through the window. It had done so, as Mr. Wimpenny had pointed out, on the question of controls.

When power controls came in, people said "We can forget about these hinge moments and so on," but now they saw that they must come back to it all. This meant that they might have quite a way to go, because they

had a much tougher problem in the transonic characteristics of controls than in the subsonic.

Probably no one would agree with his next remarks. He would not rule out completely the possibility of operating all-moving tailplanes or wing tips manually. That might be regarded as a very bold statement, but they had got to get back to the fight to balance controls aerodynamically, if only to ease their power requirements for the power-operated controls. So if part of the way, why not try for the whole lot?

He hoped that more people in the Industry were in agreement with Mr. Wimpenny on the question of alternative forms of control. He was sure that if they were, this would give the necessary impetus to the research work, which was necessary in this field.

He thought there was much more to be learnt about oscillatory derivatives before that problem could be regarded as one of the less important ones. To understand fully the transonic effects here, as in most of the other transonic characteristics, they must aim, as Mr. Wimpenny pointed out, at a more thorough knowledge of the boundary layer—shock interaction. Theory gave some clues on how wing plan form affected the damping, but he did not know where Mr. Wimpenny got his information on the effect of thickness ratio, although he would agree that on general grounds one would expect some improvement from a thinner wing.

Another point in connection with the oscillatory derivatives was that a preliminary examination which he had made of the available test data from models did not suggest that those yet gave reliability as regards quantitative results.

He was a little puzzled by the statement which placed torsional stiffness as the prime consideration. He was sure that could not be true of all the four problems listed—at least, for swept wings.

Was it fair to blame the aerodynamicist for the state of uncertainty in this no-man's land of aeroelastics? Although, no doubt, there was a lot to be done on the aerodynamic side, they were on at least as good a footing as the structural people. Perhaps somebody more qualified than he could take up this point.

Could Mr. Wimpenny give a comparison in the cost in man power or in man hours of keeping flying power-operated controls as against manually-operated ones?

MR. WIMPENNY: There was just one point that he would like to make. He heartily agreed with what so many people had said about the automatic stability question, and he certainly did not advocate letting the aerodynamics fly out of the window. Of course, they had to be most careful about it. His main point was that it gave a greater flexibility of choice; and provided that it was used properly, he thought it was a very valuable thing.

W. PINSKER (Royal Aircraft Establishment) *Contributed*: Mr. Wimpenny suggested that the advent of the auto-stabiliser would permit the designer in future to provide only a minimum of inherent aircraft stability and to let the auto-stabiliser look after the perfection of the aircraft's handling characteristics. This view seemed to have caused some alarm, judging from the discussion, and this alarm would surely be justified if Mr. Wimpenny's suggestion implied that in future he would give them less inherent stability and more black boxes instead. He did not think that was likely to happen. In point of fact, with modern high performance aircraft at any rate they were at present only getting very marginal stability and relaxation below this standard would hardly have been in Mr. Wimpenny's mind. On the other hand, the auto-stabiliser as conceived by Dr. Doetsch and developed in this country was, of course, meant to assist the designer in installing into an aircraft a degree of flying comfort and handling efficiency which it was practically impossible to achieve by aerodynamic means even at an excessive price. Even to achieve only minimum aircraft stability the designer of future aircraft would have to make greater efforts than ever before and there was little prospect indeed for Mr. Wimpenny to sit back on his well-earned laurels.

He felt that with the majority of modern aircraft designs it was virtually impossible to achieve both oscillatory and spiral stability. He would, therefore, like to have Mr. Wimpenny's views on his experience on the practical importance of spiral stability. Was the "trimmability" of modern aircraft, in particular with power controls, sufficient to allow a real benefit to be obtained from better spiral stability?

In the $l_v - n_v$ diagram there would always appear a stable region which might, however, be associated with excessive values of both derivatives. Large n_v—apart from its unpopular demand on fin volume—would shorten the period of the lateral oscillation and thus spoil the pilot's chances of effective control and would also result in a generally rough ride in turbulent air. l_v was detrimental to aileron power, as Mr. Wimpenny had indicated in Fig. 11, and it might also determine the size of the ailerons required to hold an aircraft level during cross wind approaches. Ailerons designed to satisfy those requirements might become structurally unwieldy and might also be too powerful at high speeds on a small span aircraft.

Product of inertia was another crucial parameter affecting lateral oscillatory stability and it was one of the limitations of the $l_v - n_v$ diagram that it did not reveal its significance. He would like to see designers become more inertia conscious (that was trying to keep inertia and roll down and attempt positive incidence of the principal inertia axis). More attention early in the design might show unexpected improvements.

He fully endorsed Mr. Wimpenny's plea for more research and in particular, more experiments. He would like to see, however, some of the present activities on apparently mathematically very satisfying problems diverted to more important ones. Surely, for instance, there was sufficient literature on supersonic l_p to satisfy generations of designers, whereas they still had to rely largely on intuition for the estimation even of subsonic values of n_v and n_r. Mr. Wimpenny had drawn attention to the fuselage contribution to damping in yaw, which appeared to weigh heavily in the balance of stability with many modern designs. Simple experiments could give all the information needed.

Another promising method of obtaining aerodynamic data in flight was the transient response technique. It appealed for its conspicuously modest demands on flying time, but it required accuracies of flight recording which at present were not available in this country and investments in this field seemed well worth making.

A. S. TAYLOR (Royal Aircraft Establishment, *Assoc. Fellow*) *Contributed*: He would like to take up Mr. Thomas's point in querying the lecturer's assertion that the accuracy of aeroelastic calculations was limited mostly by doubts on the aerodynamic derivatives used, rather than by ignorance of the structural parameters involved.

Certainly they could not yet specify their aerodynamic derivatives with a hundred per cent. accuracy, but at least, they had available plausible theoretical methods of calculating them in both the subsonic and supersonic speeds.

Were they in any better (or even as good) a position for specifying the elastic properties of a swept wing or tailplane in the form most suitable for aeroelastic stability and control calculations, namely a pair of flexibility matrices associated with what Broadbent had called the "Q_0-line"?

When an aircraft had been built, the provision of such data should be a relatively simple matter necessitating only a comprehensive programme of stiffness tests using an adequate number of loading and measuring stations. It would greatly assist the research worker in the aeroelastic field, if data were available in this form for representative wings of the various types now in existence, but hitherto firms did not seem to have appreciated the need to make the necessary tests.

Could they therefore ask the powers-that-be, within the firms and at the Ministry, to arrange to get this much needed information, coupling this request with a plea that some structural specialist should cope with the (presumably) more difficult problem of telling them just how to calculate those structural influence coefficients for swept wings, with reasonable accuracy, in the design—or even the project—stage.

Regarding Mr. Wimpenny's reference to the increasing use of what he called the "explicit" method in dealing with aeroelastic calculations and the like, he was interested to know the extent to which firms in general were reconciled to the necessity for engaging in complicated calculations with, or without, mechanical aids. Having devised an "exact," but complicated, method of solving a particular problem, could the research worker rest content that the Industry would gladly accept this method as providing exercise for its electronic brains, or must he still look for a simpler, if less accurate, solution?

A. B. HAINES (Royal Aircraft Establishment, *Assoc. Fellow*) *Contributed*: Mr. Wimpenny had referred to the difficulty of scale effect when interpreting the results of wind-tunnel tests to predict the full-scale aircraft performance, particularly when considering phenomena associated with a separation of the boundary layer, whether at high incidence, at low Mach number or induced by a shock wave at high Mach number. With regard to the latter, various tests had shown that the shock wave-boundary layer interaction was not the same when the boundary layer was laminar ahead of the region of this interaction—as in most wind-tunnel tests—as when the approaching boundary layer was turbulent, as would generally be the case in flight. Also, it was doubtful whether some of the methods that had been used for fixing transition far forward on the model were successful in producing the same pressure distribution farther back in the vicinity of the shock wave as if transition had occurred ahead naturally, rather than being induced artificially, *e.g.* the position of the shock wave along the chord might not be the same. However, despite these difficulties, would not Mr. Wimpenny agree that in general, where correlation between flight and tunnel at high Mach number had been possible, the order of agreement had been surprisingly good? As an example of this, a wind-tunnel test at say, $R = 2 \times 10^6$ on a swept-back wing of moderate aspect ratio often appeared to predict with fair accuracy the value of C_L at which the nose-up change in pitching moment occurred at supercritical Mach numbers, owing to the effects of a shock-induced separation on the outer sections of the wing, even though for a wing with at any rate, a thickness/chord ratio of 10 per cent., such a test might be quite misleading in predicting that value of C_L for low Mach numbers. Despite this somewhat comforting conclusion, it remained true that every possible opportunity should be seized for obtaining more instances of flight-tunnel correlation and also, every effort should be made to devise means of making the model tests more representative, *e.g.* by research into the effects of roughening the leading edge.

Another difficulty in intepreting wind tunnel tests was that of deciding whether the observed aerodynamic characteristics were likely to be acceptable in flight or not. An example was: suppose a test had been made on a stall fence fitted to a model of a swept-back wing and the results had indicated that some reduction in longitudinal stability still occurred above about the same value of C_L as for the wing without fence, but that the magnitude of this reduction in stability had been considerably reduced until an appreciably higher value of C_L was reached. At this higher value of C_L, it would be common to find a very abrupt nose-up change in pitching moment and reduction in lift-curve slope. In interpreting such results, two questions were important:

(*i*) What smaller reduction in stability at the first critical value of C_L would be acceptable in flight, and (*ii*) assuming that this was acceptable, was the consequent apparent increase in usable C_L to be counted as an improvement if it were only obtained at the expense of very unpleasant characteristics at higher incidences?

Clearly, to be able to answer those questions, there should again be close liaison between those engaged in flight and wind tunnel testing and also, as far as possible, not merely overall results should be obtained in the wind tunnel but tests should be made to understand the basic nature of the changes in flow.

Mr. Wimpenny suggested that there was usually a

discrepancy of 0·02 to 0·03$\bar{c}$ between the position of the neutral point as determined from the wind tunnel and from the aircraft in flight. Was this possibly rash generalisation, based on experience with propeller-driven or jet aircraft or, with both? Also, had it been found whether the models were mounted on struts or by a single sting from the rear?

Mr. Wimpenny mentioned that it might be desirable to introduce a nose-up kink in the curve of C_m against Mach number just above the critical Mach number prior to the usual nose-down destabilising trend. This point was surely only of crucial importance for a civil aircraft and should be met by artificial means, rather than by aerodynamic design. Admittedly there had been cases in which aircraft had shown this characteristic but it merely reflected the presence of some poor feature in the design, such as a premature separation of flow in a wing-nacelle junction. In fact, one aim of a good high-speed aircraft design might be to ensure a steady rearward movement of the aerodynamic centre from its subsonic to supersonic position.

Mr. Wimpenny appeared somewhat complacent when discussing the use of stall fences to cure the effects of tip stalling on a swept-back wing at both low and possibly high, Mach numbers. It was true that it was relatively easy to experiment with fences of different heights and chordwise extents at different positions along the span of the wing of a prototype aircraft. It was also true that in certain cases fences had yielded useful results, that they might be acceptable for wings of relatively small sweep and that they might be essential for swept wings of high aspect ratio. Any fence, however, implied a significant penalty in drag, particularly at high Mach number when shock waves would form prematurely on the wing on both sides of the fence. They should therefore aim at the ultimate elimination of the fence as a device to improve the pitching moments at high C_L on swept wings of moderate aspect ratio.

At low and "subcritical" Mach numbers (up to perhaps about 0·05 less than the Mach number at which the steep increase of drag with Mach number started for low values of C_L), improvements should be sought rather from suitable changes to the wing section in the vicinity of the leading edge. These changes should probably be graded across the span of the wing in some continuous manner with no abrupt discontinuities in leading edge in plan view; if the latter were incorporated, then the accompanying changes in section shape would need to be varied in a more complicated manner.

At higher Mach numbers, the problem had so far appeared less tractable except for the conclusion that thinning the outer wing sections should give a useful increase in the values of C_L at which a reduction in longitudinal stability occurred. Thinning should also avoid the risk of buffeting or wing dropping at high Mach numbers.

Ultimately, it might be possible to find other means of improving the characteristics under these conditions: for example, it might be found possible to alleviate the effects of a shock-induced separation on the upper surface of the wing by some change in the aerofoil shape towards the trailing edge or alternatively, to lessen the risk of such a separation by suitably designed vortex generators, but in both respects further experimental work, both in flight and in the wind tunnel, was needed. Considerable progress in all these directions throughout the Mach number range were currently being made.

MR. WIMPENNY'S REPLY

MR. HANDEL DAVIES: He would agree that feel requirements were a function of the type of aircraft and its role, and that the small high speed fighter might present a more exacting problem than the large transport. At the same time he thought it likely that criticisms of some existing feel systems related quite a lot to imperfections which were mechanical in origin, *i.e.* backlash, friction, high break-out forces and so on; and this might have confused the issue on the more fundamental feel problem of whether, or how, feel should vary with speed. Thus until these imperfections had been overcome he did not think they should take it for granted that they needed the theoretical ideals in feel with their attendant mechanical complications. Another relevant factor was the control concerned. To start with the elevator controls the most important force on an aeroplane, the lift force—and it was imperative that the pilot should feel happy about his control here—it was almost certainly the control on which there was the strongest case for some elaboration in the artificial feel. The aileron was next in importance; here variations with speed were less marked than on the elevator and there seemed every reason to retain a simple spring feel on it, while the rudder being little used at speed did not seem to warrant complication because of pilots feel alone. They must, of course, guard against over-stressing the aeroplane with a power control; on the aileron and rudder this could sometimes be done by carefully matching the maximum booster effort to the largest manoeuvre required; should this prove impracticable in a particular case then some "q" feel or variant thereof might be required to avoid over-stressing at high speeds.

Figure 1 indicated that a pure force/no movement control would be possible, but he questioned whether it would be the most desirable. It was fairly certain that stick forces played a bigger part in feel than did stick movements; and provided that the forces lay within reasonable limits, large percentage variations in stick movements could be tolerated, as indicated by the "acceptable" boundary of Fig. 1. However, he could not help feeling that a certain amount of movement was liked by pilots, especially to initiate a manoeuvre; this arose perhaps from the psychological parallel between a stick movement and the desired movement of the aeroplane. Also, the preference arising from actually sitting in an aeroplane with the runway fast approaching might well differ from that indicated by a laboratory experiment. He agreed, of course, with Mr. Davies that there was in any case a fundamental engineering reason for having some stick movement—if they started with a pure stick force only, then a complicated mechanism would be needed to transform it to a movement. Also,

he was sure a pilot would feel much happier about a system in which he was connected mechanically with his control, rather than working through some servo device. He would prefer to consider the whole question the other way round; granted that the stick forces were first made right, what then was the objection if a moderate amount of stick movement were associated with them?

They had, of course, to be careful about relaxing stability requirements with auto-stabilisation and he would be the last to suggest a facile optimism on this matter. The main point was that they could work to the two standards of the auto-stabiliser on and off, but they would have to feel their way carefully on where to draw the line.

He would consider a divergent Dutch Roll on the approach to be acceptable when the auto-stabiliser had failed, provided that the period was long enough to enable the pilot to control the aircraft with rudder; there had been cases when the pilot did not know the oscillation to be unstable until a special test to measure it was done.

He endorsed entirely the remarks on production tolerances.

MR. LEE: It was encouraging to find that they seemed to have been thinking along the same lines as Mr. Lee's firm, and he was particularly interested to hear of the inter-connected ailerons and rudders on the Victor.

Regarding the effect of wing bending stiffness on aileron reversal, he did think that flexural stiffness had an important influence, assuming that by flexural stiffness they meant stiffness in the plane of the predominantly structural axis and not in transverse planes. At the reversal speed the loading conditions on the wing were as follows:—

(*i*) A couple from the aileron deflection. This was the dominant loading, and acted about an axis at right angles to the *line of flight*.

(*ii*) Shear forces arising from the imperfect cancellation of the loads due to aileron deflection and the wing distortion. Bending moments could exist along the span but must be zero at the aircraft centre line.

The bending arising from (*ii*) was small but not necessarily negligible—it might be unwise to generalise here too much as it arose from the difference of large quantities, but in several cases they had investigated it could affect reversal speed adversely to the order of 5 to 7 per cent. The much larger bending effect came from the resolution of the line of flight torque about and along the flexural axis; the latter component of torque produced bending deflections which, when resolved back into the line of flight incidences, could account for up to 40 per cent. of the total line of flight incidence change. Thus an increase in purely flexural stiffness by, *e.g.* increasing the stringers, should increase the reversal speed. Of course one would not do it in that way as it would usually be better to increase the torsional stiffness at the same time by putting as much material as possible into the skin, but it remained true that flexural stiffness must be allowed for.

Those arguments applied if they considered the structure behaving in a simple way with a flexural axis running somewhere along the 40 per cent. chord line; it was then convenient to resolve the aerodynamic forces, which were dependent on line of flight incidences, about the flexural axis.

They might say that the flexural axis concept was incorrect—true it did not allow for the effect of root conditions and cut outs, but the significant structural distortions were more in the middle and outer parts of the wing where the structure would behave more simply, and he thought enough of the simple flexural axis principle would remain to afford an easily understood explanation of the principles of how the wing would distort, unless the structure be of extreme form, as on a delta. In the particular cases investigated they had also made allowances for the influence of the wing root on the structural distortions.

He had discussed the alternative approach of considering the structure as having a certain stiffness in the line of flight only, in his reply to Professor Collar.

PROFESSOR COLLAR: From the aerodynamic point of view he agreed that it was more convenient to think of aileron reversal as demanding a certain torsional stiffness taken in a section containing the wind direction. Under those conditions the torsional stiffness required would decrease for a swept-back wing, due presumably to a drop in the dominant aerodynamic parameter, $(a_1 m)/a_2$, with sweepback. However, this only brought up the effect of bending stiffness in a different guise. Thus the torsional stiffness in the line of flight sections would depend upon a combination of the conventional bending and torsional stiffness where the latter were measured about the swept structural axes, and it was to these axes that the terms bending and torsional stiffness in his main text referred. As explained in his reply to Mr. Lee there remained significant effects of bending stiffness, and so he did not think that working in terms of line of flight stiffnesses was really more than a convenience in technique; and to dismiss bending stiffness (reckoned along the swept structural axis) as a red herring would not be justified.

The technique of pressure plotting on a model which could be given any desired distortion was very valuable, and as the loading due to distortion was half of the reversal problem it was necessary to know it accurately. The only point he felt was that load gradings due to incidence changes were more amenable to theory than those due to control surface deflection, and could furthermore be checked for the related case of a uniform change of incidence. He would have thought that the load distributions due to control deflections should have had first priority but some tests on the load gradings due to twist for a few representative plan forms would also be worth while.

MR. HARDMAN: Fig. 4 assumed derivatives invariant with M, and the effects shown would usually be aggravated beyond the critical Mach number. The effect of aircraft inertia existed, but primarily affected the transient incidence adjustment which followed a

sudden passenger movement. However, even with the inertia lag included the incidence response was soon over on an aeroplane with positive manoeuvre stability (in the order of one sec.), and the motion which caused the trouble was essentially the much slower phugoid oscillation whose period was of the order of one min. or more, and primarily depended on aircraft true speed. Fig. 4 referred to the speed changes in the phugoid oscillation. Motions involving speed changes were, to a considerable extent, determined by the inter-change of kinetic and potential energy and, for this reason, were relatively slow. If an aircraft were statically unstable at transonic speeds the rate of divergence would be relatively slow and give the pilot time to react, but this was due not to the effect of pitching moment of inertia, but to the magnitude of the gravity forces acting on the mass of the aeroplane.

He agreed with the remarks about lateral stability; while on the last point it was true that if a swept wing lost manoeuvre stability due to aeroelastic distortion there would be a simultaneous increase in the tail contribution to stability. The values quoted in the text, however, allowed for this and for tails of moderate $\overline{V}$ (around 0·40) it was quite possible for there still to be a resultant loss in stability.

MR. VESSEY: The time to roll a specified number of degrees did appear a more logical measure of rolling power than the helix angle, but it appeared unlikely that it would be possible to ascribe a unique value to it as there was a fundamental effect of aircraft size—pilots accepted this and there must, therefore, be some flexibility in what they would accept. Time to bank involved both acceleration into as well as steady rate of roll, *i.e.* it allowed for lag effects which might be good as pilots objected particularly to lag.

He found no statement in his text saying that aerodynamic centre and downwash were the predominant causes of the basic nose down moment at transonic speeds—the implied reference to those two terms specifically referred to the effect of local root modifications only. He would agree that those were major factors but others had to be considered and the final trim change might be the difference between several quantities. Before saying that nothing could be done to control it, he felt that analysis of existing data and wind tunnel tests on root modifications might well show that the situation was not so far beyond their control as appeared at present.

He hoped that he did not convey the impression that the pilot was a *poor* form of servo-mechanism—and he agreed with Mr. Vessey's further remarks—the point was that it was difficult to express the pilot's characteristics analytically—a necessary preliminary to any mathematical analysis.

MR. MEREDITH: Mr. Meredith had, he thought, hit the nail on the head in his remarks about break-out forces. He might only add that it had long been the practice on large civil aircraft to use the trim even when they had manual controls—it made for smoother flying and in any case the controls were probably very heavy to move directly. This practice applied mostly around the cruising speed, at low speeds it was of course necessary to use the stick directly, but as the movements required were then coarser, the effect of the break-out force was relatively less. He concurred with the remarks on auto-stabilisers and would assure him that the aircraft designer did not expect the auto-stabiliser to be infallible, certainly in the present state of the art.

Longitudinal stability was fundamentally concerned with the effects of changes of incidence. Incidence changes in flight occurred under two conditions; firstly quick changes of incidence with little change of speed (*e.g.* an up gust or when the pilot manoeuvred and put "g" on the aeroplane), and secondly when accompanying the change of incidence there was a change of speed so as to keep 1 "g" on the aeroplane—the latter changes were relatively slow. Manoeuvre stability, as the name implied, referred to the stability with respect to changes of incidence at approximately constant speed; static stability was in respect of changes of incidence with simultaneous adjustment of speed keeping approximately 1 "g" on the aeroplane.

"Buzz" referred to an oscillation which could occur in one degree of freedom only; for such a system to vibrate it was necessary for there to be a lag in the development of the air forces following a displacement in that degree of freedom, *i.e.* the trouble was fundamentally aerodynamic. Such lags could and did occur at any speed, but in practice they only became serious either at the stall or at transonic speeds. For example a control free to rotate about its hinge line had thereby one degree of freedom, and compressibility buzz could be caused if the formation of shock waves caused a lag in the development of circulation when the control was deflected. Such buzzes usually built up to some self-limiting amplitude as the aerodynamics were non-linear. While they could be serious they were rarely catastrophic.

Flutter essentially involved the coupling of two or more degrees of freedom, *e.g.* control rotation and wing bending, and the instability was caused by a lag of one motion with respect to the other, the lag being due to combinations of aerodynamic, inertia, and elastic couplings between the two degrees of freedom. Once the critical speed was exceeded the oscillation might well diverge exponentially and there might then be a grave risk of catastrophic structural failure.

It would indeed be *possible* to add a further term to the gear changing mechanism to cover transonic speeds but one would need a pretty complicated box of tricks to do that as well.

DR. DOETSCH: He doubted whether it was fair to compare the standard of damping of the short period pitching oscillation with that of the lateral oscillation. The pilot might be more critical of unwanted pitching oscillations because they would shake him about much more. At the same time it was difficult to lay down a hard and fast limit in the present state of knowledge—a pilot would probably comment unfavourably on anything much in the way of an over-shoot in pitch, *i.e.* more than about 0·3 cycles to halve amplitude, but the

absolute limit of acceptability would probably correspond to less damping than this.

He might add here that although pilots had commented on reduced damping at high altitudes it did not follow that the limits of acceptability depended on the damping alone; it might well be that the "touchiness" complained of was due partly to over-large controls or too high a stick gearing, and attention to these latter items might permit a lower standard of damping.

Increase in pitching moment of inertia would reduce the absolute damping of the short period oscillation (*i.e.* the time to damp), but could increase the relative damping (*i.e.* the cycles to damp), provided that the tail contribution to damping was basically small. The improvement in relative damping was at the expense of slowing down the response of the aeroplane. A similar effect on the relative damping could be caused by merely reducing the manoeuvre margin, but after a certain point would not be acceptable owing to large responses to gusts and control movements. He could not help wondering whether some similar disadvantage, such as a loss of crispness in response to the control, might possibly attend the improvement in relative damping when the moment of inertia was increased. It was an interesting point and, as far as he could see, might well represent a net advantage.

It was true that increasing fin size to improve Dutch Roll was very effective at first, but then followed a law of diminishing returns, especially if they were considering relative damping instead of the stability boundaries. The benefits or otherwise of a large fin did then depend considerably upon the particular case—parameters such as n_p, l_p, roll inertia, and the product of inertia between roll and yaw exerted complicated interacting effects which made it difficult to generalise on the answers. Each case must be looked into individually, and one would work out actual dampings, as well as the stability boundaries.

As a result of investigating the relative effects of spiral instability and lateral divergence due to an aileron trim defficiency he agreed that the latter appeared the more significant and that he might have overemphasised the importance of spiral stability.

For an aeroplane flying at a true air speed of 500 m.p.h. and with an amount of spiral instability equivalent to a dihedral, 2° less than that required for neutral spiral stability gave a time to double amplitude of about 10 minutes. On the other hand a 1/10° out of trim on an aileron could produce a rate of roll of the order of $\frac{1}{2}$°/sec., and this latter effect would certainly be the more embarrassing. If they assumed that a reasonable standard of wander from a desired course was a change of heading of 10 degrees in 2 minutes, *i.e.* the pilot would have to intervene every two minutes to keep his course within ±10°, then the aileron would have to be trimmed to within about 1/150°. This value was so small that it was difficult to see how even an aeroplane with manual controls could be trimmed to this accuracy as the presence of circuit friction would be expected to hold control displacements larger than this. For times of this order the motion causing trouble was one of almost pure rolling under the out of trim moment from the aileron. For longer times, the motion became affected by spiral stability, and for periods of the order 5 to 10 minutes the motion was considerably modified; if spirally unstable, the rate of roll would increase considerably, but if spirally stable the rate of roll would eventually drop to zero and the aircraft stabilise at some angle of bank. The greater the spiral stability the quicker would the rolling velocity subside to zero and the smaller the equilibrium angle of bank; with spiral stability given by a dihedral 2° above that for neutral spiral stability it appeared that the bank angle might settle in about 12 minutes. This would only assume some practical importance if the aileron had been set so accurately that it had not already produced excessive bank before 12 minutes, and as for this the out of trim aileron angle would be so minute it appeared on the high speed class of aeroplane that trimming considerations over-rode. Thus the indications were that the pilot was unlikely to be able to leave the aeroplane alone for long unless a very fine degree of trim was provided. For low speed aeroplanes spiral stability was of greater importance as the time scale of the spiral motions was reduced with the speed. The apparent importance of extreme accuracy in aileron trimming was something he had not appreciated and he was still rather surprised that it had not been more generally known before, or thrown up by experience as a matter demanding careful attention. It would be interesting to hear other firms' experience on this as it seemed to be relevant to manual, as well as power, controls, although considerably aggravated by the latter. He was grateful for the matter having been raised, and in view of this situation he would agree that the dihedral should be determined more from Dutch Roll considerations.

While he agreed that it was not the function of auto-stabilisation to make a dangerous aeroplane safe, he certainly did not think that the case of curing a gentle nose down trim change at transonic speeds fell into that category; on the contrary it represented a strong case for auto-stability, the rate of divergence was fairly slow but the provision of inherent static stability might be difficult. There would, of course, be no question of using auto-stabilisation to cure any manoeuvre instability or an undamped short period oscillation.

MR. LAWSON: The diagram of Fig. 1 was intended to refer to one condition of weight and c.g. only. The problem posed by varying weight and c.g. was with them anyway, irrespective of power controls.

The simple inertia weight did the very thing Mr. Lawson asked for, being quite independent of weight, c.g., speed or M. There were two snags with it: it might interfere with the dynamic stability of the short period oscillation, particularly if there were solid friction or stiction in the input circuit to the booster; also the pilot could beat it by heaving quickly on the stick, and solid friction could mask its effect. However, if solid friction in the input circuit could be kept low it might be possible to secure quite a good feel by introducing a velocity damping unit, possibly variant with speed, together with the "g" weight—the velocity damping to discourage the

pilot from putting the control on too quickly, with the inertia weight effect following up in the steady case.

Calculations based on applying a control and then holding it steadily in the displaced position, showed little benefit for increasing control rates of operation to as high as 40°/sec., but pilots did object to rates as low as the 20°/sec. indicated by such calculations. The objection might be partly psychological in that they did not like to feel they were up against a limit and one set of American flight tests (ref. N.A.C.A. T.N.2238) suggested that the pilot would accept quite low rates of control movement, provided that he could move the *stick* quickly, *i.e.* he was not directly connected with the elevator—he thought, however that this conclusion should be taken with a grain of salt as that sort of set up sounded like an effective lag-producing device which, in many cases was objected to violently. On the other hand it might also be that applying the control in this way did not correspond very well with the type of control movement used during say, an approach to land where, although the *mean* control used might not be large, continual large displacements of control about this mean were being made; the pilot was "feeling" his way in using continual corrections, and by making a large movement on the control and then quickly returning it to near its starting position, he could get a quicker response from the aircraft than if he were to use a step displacement only—under these conditions the ability to use a high rate of control application might represent a real advantage.

He rather doubted whether a nose up change of trim due to lowering flaps was a serious enough factor to influence tail size. The tail was primarily a damping and control organ and he heartily agreed that it should not be made large just to get stability; it was inefficient to do so, for as Mr. Lawson said, its effect would vary considerably with flight condition and set a more severe overall control problem as a result.

With an all-moving tailplane operated by a servodyne type of booster, the rate of operation was under the pilot's direct control and could be made as fast or as slow as desired—the latter case could be particularly slow if the tailplane were moved through the trimmer.

MR. THOMAS: He was not aware of pointing out that control aerodynamics had actually flown out of the window; in fact he thought he was pointing out the opposite; and that control hinge moments today certainly remained an important problem with power controls. Mr. Thomas was quite right in expecting disagreement on the use of manually operated all-moving tips—there was a world of difference between securing a reasonable reduction of hinge moments to say, 20 or 30 per cent. of unbalanced values, and the close balance to 5 per cent. or less required for manual controls.

Torsional stiffness was a dominant parameter in flutter; in aileron reversal and wing divergence it was more important than the flexural stiffness, although the latter had a significant influence on a swept wing; while only in the fore and aft stability of a high aspect swept wing did flexural stiffness play an equal or more important part.

On the question of the man hours required for maintaining power controls, he regretted it was not possible to obtain any specific figures for this item of maintenance alone. The operators checked and overhauled the relevant hydraulic components as a matter of routine; in this respect that only represented an extension of techniques already well established for maintaining other hydraulic systems such as the flaps, and undercarriage. There was no denying, however, that the extra hydraulic system for the flying controls did represent an added source of maintenance, but the percentage extra in relation to the maintenance work as a whole in no way caused concern or particular comment.

MR. PINSKER: He agreed with the general lines of the remarks on auto-stabilisation and spiral stability, and had in effect replied to them under questions raised by previous contributors.

The remarks on ailerons and excessive I_v served to underline how complex were the problems of achieving a satisfactory all-round compromise between the various requirements of directional and lateral stability and control. Much of the difficulty was associated with the use of large sweepback and was some reason for carefully considering the thin straight wing as an alternative for supersonic flight. Performance indications suggested a slight inferiority to the swept wing at low supersonic speeds, but a superiority at higher supersonic speeds, and if there were not a lot in it then the improved stability and control of the straight wing might turn the scales in its favour.

Roll inertia and product of inertia had a large influence on the Dutch Roll damping, but apart from not placing external stores far outboard, the designer could only make major changes in the rolling inertia by major changes in the external and internal layout of his aeroplane. There were very many other practical considerations which went in to settling such matters and he thought all that could be said was that the designer should add these inertias to the list of factors to be considered.

MR. TAYLOR: Their experience at de Havillands was that for control reversal and stability effects they were a good deal happier about the structural side of the problem than the aerodynamic side. A variety of estimates had been done for sweepback up to about 40°, taper ratios up to about 3·5 : 1 and aspect ratios down to about 3·0. They had used normal strain energy methods to estimate the elastic characteristics of the wing and obtain results which were in good agreement with ground stiffness tests; the process was then extended to the estimation of reversal speeds, using an iterative process. For such wings the most significant deflections came from the middle and outer parts of the wing where the structure was usually fairly simple; thus possible errors in the structurally more complex root regions with fuselage, engine and wheel well cut outs, did not have a very large effect on the reversal speeds. They had not

evaluated the flexibility matrices, but if these were used the accuracy achieved in the final answer would be about the same.

On the aerodynamic side there were quite good theoretical methods available to deal with the effects of incidence changes due to wing twist, but there was still a lack of the corresponding data for control deflections. In addition, the critical design cases were usually associated with peak values of the derivatives at around the critical Mach number—theory was not at its best here and one had to use what little experimental pressure plotting data there was to establish both peak values of the control derivatives and their spanwise distribution. More theoretical work and comparisons with experimental data were required in the region of high subsonic Mach number.

When it came to delta wings the structural position did become more difficult; the best method of solution would be using flexibility matrices, but this would necessitate the use of a digital computing machine to be effective and on this they had as yet no experience. A similar situation applied to the estimation of normal modes; matrix methods with a digital computing machine were needed to achieve sufficient accuracy.

Firms were, he thought, becoming increasingly reconciled to the need for complicated calculations, and while they were in a relatively early stage just now, it was likely in a few years time that digital computing machines would be in general use for aeroelastic problems and the like—and the suggestion of pooling information on how to estimate structural influence coefficients would then be valuable. He would still say, however, that it was desirable to have a simpler, less accurate solution available, mainly because the more complicated the calculation the more difficult it was to keep track of the physical connection between the things they fed into the machine and the answer they got out at the other end—one was more likely to be able to see the connection in a simple calculation, and he believed in principle that, if at all possible, they should use simple approximate methods as a rough check on a complex calculation—the latter *could* go off the rails.

MR. HAINES: He would not go so far as to say that agreement between high-speed model and flight tests was surprisingly good—it all depended on what standard of agreement one was going to be surprised at! If Mr. Haines meant that the tunnel was going to give a qualitative answer as to the type of high M effects, then he would agree (provided that a rear sting support was used), but if the model were to be used for numerical analysis he would not give a general O.K. to present techniques—they certainly gave the designer a valuable indication and were well worth while, but as Mr. Haines said, there remained room for improvement. Fundamental research at the N.P.L. had shown that there were important effects of boundary layer/shock inter-actions which depended particularly on whether the boundary layer was laminar or turbulent, so any work on means of simulating high R.N. by promoting early transition on the model was very important. He would have thought it good to aim at checking all models, with and without artificial transition, to try to build up some experience on the matter. The tests on the Comet model did show quite noticeable differences transition wires on and off, of which the tests with wires on, agreed better with flight.

The question on interpreting wind tunnel tests into flight characteristics was an important one. On the particular example cited he would not think it an improvement to exchange early buffet and mild instability for very unpleasant characteristics at the stall itself. The general question was that raised in the first part of the Lecture—they did not know enough about what were limiting flight characteristics, and with present methods a lot of time had to be spent in expensive test flying to find the answer. Flight tests must remain the final arbiter, but there was room for more work, first in calculating the dynamic flight characteristics from the measured steady state derivatives, secondly in comparing these estimates with the measured flight behaviour, thirdly and most important of all, in establishing a correlation between these and what the pilot would or would not accept.

The quoted loss of stability model to full scale was a fairly broad generalisation based on tests with and without propeller and with both types of model support —they had had a particular example recently of a jet aircraft which was tested with a sting support, but one got an impression that other firms had had a similar experience. Some years ago an analysis was made of the effects of Reynolds Number on longitudinal stability (ref. R.A.E. Tech. Note Aero. 1104) and the general indication was that full-scale R.N. were more likely to result in a loss, than a gain, in stability, due mainly to effects on the tail. It would be valuable to have such an analysis brought up to date.

The importance of not having a nose down trim change just above the cruising Mach number was certainly not confined to civil aircraft but applied to all aircraft which had to cruise for long periods of time—bombers and night fighters came into that category as well. The aerodynamic changes needed, need not of necessity correspond to some poor feature and he thought it was worth attempting an inherent cure; but failing this auto-stabilisation should not prove difficult.

He thought they agreed really on the use of fences—it would be preferable to design in other more fundamental cures for tip stalling. This they did on the Comet —the usefulness of the fence however, was in putting the finishing touches to something which was nearly there without them, and for this purpose they had found them very useful. The drag penalty on a long range aircraft was noticeable and one would like to be without it, but it was a price worth paying if the requisite good stalling properties could not be achieved without the fences. On fighter aircraft the drag penalty was not very significant.

A Method for Determining the "Safe" Life of an Aircraft Wing From Fatigue Test Results

by

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1. Introduction

It is now the usual practice in Great Britain to estimate the "safe" life of an aircraft wing from fatigue tests which are carried out on specimens representing the more highly stressed portions of the tension boom or booms. Such tests are designed to produce a tensile load in the boom equal to that produced in steady flight, and superimposed on this, a small fluctuating load corresponding to that which would be produced by an up gust immediately followed by a down gust of the same magnitude. By means of a gust frequency curve, an *S-N* curve, and assuming that the cumulative damage rule is valid, it is possible to relate the tests carried out at one load level to the gusts of varying magnitude which the aircraft is likely to encounter in flight. In this way it is possible to convert from test reversals to failure to life in flying hours[1].

Fatigue tests, even of this simple nature, are expensive, and so only a few specimens of any one portion of boom can be tested. This fact, and the large scatter which is typical of fatigue results, has led to the adoption of an arbitrary factor which is used to convert the mean of the reversals to failure to the value of the reversals to failure on which the "safe" life can be based.

It is the purpose of this paper to suggest an alternative to this arbitrary scatter factor. The basis of the method suggested is the assumption that fatigue test results can be represented by a log-normal distribution and statistical methods, applicable to small samples, are employed to determine the probability of a wing boom being capable of withstanding a particular number of reversals to failure. The "safe" life is based on the acceptance of one wing fatigue failure in a chosen number of flying hours. The fact that fatigue failure, although it is most likely to occur at the portion of boom which is most highly stressed, can occur in another portion which is subjected to a smaller stress, is emphasised. Finally, the possible advantage in fatigue life of some box types of wing structures over wings with heavy spars is discussed.

Notation

$\overline{X}$ = mean of the population of log reversals to failure

σ = standard deviation of population of log reversals to failure

p = probability of obtaining a value of log reversals to failure less than a chosen value

U_p = standard normal deviate corresponding to a probability p

n = number of specimens tested (sample size)

x = any result of log reversals to failure

$\bar{x}$ = mean of results of log reversals to failure obtained from the sample of size n

s = standard deviation of the sample of size n

k = a value such that $\bar{x} - ks$ can be used as an estimate of the value $\overline{X} - U_p\sigma$

U_q = standard normal deviate corresponding to a probability q

p_c = probability of failure of the wing centre section boom failing before a given number of flying hours

p_1 = probability of failure of a single outer wing section 1 before this number of flying hours

p_2 = probability of failure of a single outer wing section 2 before this number of flying hours

p_3 = probability of failure of a single outer wing section 3 before this number of flying hours

P_{c1} = total probability of failure before this number of flying hours of at least one of, the centre section, or two outer wing sections 1

P_{c12} = total probability of failure before this number of flying hours of at least one of, the centre section, or the two outer wing sections 1, or the two outer wings sections 2

P_{c123} = total probability of failure before this number of flying hours of at least one of, the centre section, or the two outer wing sections 1, or the two outer wing sections 2, or the two outer wing sections 3

P_c = probability of failure before a certain number of flying hours of both front and rear spar booms in centre section of box beam.

2. Proposed Method

2.1. DISTRIBUTION OF FATIGUE TEST RESULTS

One of the difficulties in trying to establish the number of reversals on which a "safe" life may be based is the very large scatter which is usual in fatigue test results. Even with well designed aircraft structures a ratio of maximum to minimum number of reversals to failure in 10 results can be of the order of 3 to 1. In a larger sample (say 100 results) this ratio may be 5 or even 10 to 1. With some structures the scatter is still greater.

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The mean value of the reversals to failure cannot be used for the estimation of " safe " life. What is required is a minimum value, and it is also necessary to have an idea of the probability of occurence of this value. This can be done only if the results are distributed in a known way. There are not sufficient results available for any one type of aircraft specimen to be able to determine their distribution. There is, however, some evidence, obtained from simple laboratory specimens, that the distribution of reversals to failure can be represented by a log-normal curve[2, 3].

It will be assumed in the present work that the distribution of the reversals to failure of typical aircraft structures can be represented by a log-normal curve. In other words, if a very large number of reversals to failure were available for any one type of specimen and the logs of these results were taken and grouped so that a histogram might be drawn, then the result would approximate to a " normal " distribution curve.

If then $\overline{X}$ and σ were known, where

$\overline{X}$ = mean of the population of log reversals to failure

and σ = standard deviation of this population,

it would be possible to pick out any value and state the chance of obtaining a value less than this chosen value.

Table I gives a list of values with their associated probabilities.

The values 1·282, 2·326, and so on, are known as standard normal deviates $= U_p$. Fig. 1 shows clearly the significance of the value $\overline{X} - 2{\cdot}326\sigma$.

2.2. FAILURE RATE AND " SAFE " LIFE

It is not possible to eliminate completely the chance of failure of a wing due to fatigue. One can only decide what is an acceptable risk and fix the life so that this risk is not exceeded. The criterion which has been adopted in this paper for demonstrating the method in assessing " safe " life is that one wing fatigue failure is acceptable in not less than 10^7 flying hours.

Suppose then that the mean life of an aircraft (corresponding to a certain value $\overline{X}$ in log reversals to failure) is 50,000 hours. Then if a large number of aircraft were flown until they either reached 50,000 hours or suffered fatigue failure, the expected failure rate would be 1 in $2 \times 50{,}000$ hours $= 1$ in 10^5 hours $= 10^{-5}$.

Similarly, if for the same type of aircraft 10,000 hours is taken as the " safe " life and this corresponds to a 1 in 1,000 chance, the expected failure rate would be 1 in $1{,}000 \times 10{,}000 = 1$ in 10^7 hours $= 10^{-7}$,

i.e. the failure rate is given by a value for the probability of a certain wing boom failing, divided by the value of the life in hours associated with this probability.

TABLE I

Value	*Chance of obtaining a value less than that chosen*			
$\overline{X}$	1 in	2	or	$p = 0{\cdot}5$
$\overline{X} - 1{\cdot}282\,\sigma$	1 in	10	or	$p = 0{\cdot}1$
$\overline{X} - 2{\cdot}326\,\sigma$	1 in	100	or	$p = 0{\cdot}01$
$\overline{X} - 2{\cdot}878\,\sigma$	1 in	500	or	$p = 0{\cdot}002$
$\overline{X} - 3{\cdot}090\,\sigma$	1 in	1,000	or	$p = 0{\cdot}001$
$\overline{X} - 3{\cdot}291\,\sigma$	1 in	2,000	or	$p = 0{\cdot}0005$

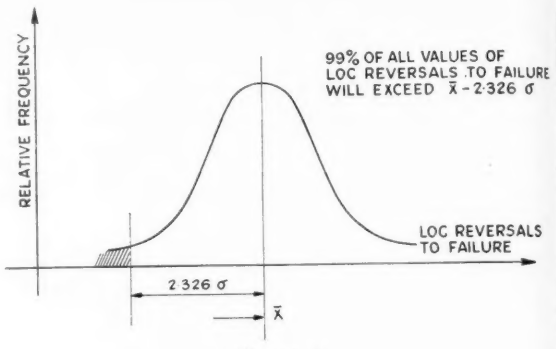
RELATIVE FREQUENCY
99% OF ALL VALUES OF LOG REVERSALS TO FAILURE WILL EXCEED $\bar{x} - 2{\cdot}326\,\sigma$
LOG REVERSALS TO FAILURE
$2{\cdot}326\,\sigma$
$\bar{x}$

FIGURE 1.

So that, if $\overline{X}$ and σ are known, a range of reversals to failure is available with their associated probabilities. The reversals can be converted to flying hours and the failure rate calculated. The " safe " life is then that life corresponding to a failure rate of 1 in 10^7 hours.

The difficulty is that $\overline{X}$ and σ are not known and only the mean and standard deviation of a small sample are available.

2.3. SMALL SAMPLES

Let x = any result of log reversals to failure

$\bar{x}$ = mean of results of log reversals to failure obtained from a sample containing n specimens

and s = standard deviation of the sample of n specimens.

Then, by definition

$$s^2 = \frac{\Sigma x^2 - (\Sigma x)^2/n}{(n-1)}.$$

Suppose that it is desired to pick out from a small sample the value of the log reversals to failure which corresponds to a probability p. The real value will be $\overline{X} - U_p\sigma$. As a crude estimate $\bar{x} - U_p s$ might be used, but the error in so doing could be very large, since $\bar{x}$ might be a bad estimate of $\overline{X}$ and similarly s could be a bad estimate of σ. In general, the smaller the value of n, the greater is the likelihood of error. From a small sample it is not possible with complete certainty to estimate $\overline{X} - U_p\sigma$ or any other value in the population. Statistical methods do however make it possible to calculate a value k corresponding to a probability p such that $\overline{X} - U_p\sigma$ will, with a certain measure of confidence, be greater than the estimate of this value $\bar{x} - ks$. The measure of confidence is usually referred to as a confidence coefficient.

In this paper the confidence coefficient which will be used is 90 per cent. This means that there is a 90 per cent. chance that $\overline{X} - U_p\sigma$ will be equal to or greater than $\bar{x} - ks$ and that there is a 10 per cent. chance that it will be less than $\bar{x} - ks$.

TABLE II
VALUES OF k
(*Confidence coefficient* 90 *per cent.*)

	Probability p					
n	0·5	0·1	0·01	0·002	0·001	0·0005
3	0·964	3·873	6·660	8·166	8·745	9·298
5	0·644	2·585	4·400	5·383	5·763	6·124
7	0·522	2·244	3·822	4·675	5·005	5·317
10	0·426	2·012	3·442	4·214	4·511	4·795
20	0·292	1·754	3·035	3·725	3·990	4·243
∞	0	1·282	2·326	2·878	3·090	3·291

It can be shown[4] that k is given by

$$k=\frac{U_p+U_q\sqrt{\left\{\frac{1}{n}\left(1-\frac{U_q^2}{2(n-1)}\right)+\frac{U_p^2}{2(n-1)}\right\}}}{1-\frac{U_q^2}{2(n-1)}},$$

where U_p = standard normal deviate corresponding to the desired probability p,
and U_q = standard normal deviate corresponding to the confidence coefficient required.

Then with a confidence coefficient of 90 per cent. (so that $U_q=1{\cdot}282$) Table II gives values of k for a range of values of p and n.

This table is used in the following way. Suppose that a value is required such that 99 per cent. of the population will be greater than this value (*i.e.* $p=0{\cdot}01$) then this value is $\bar{X}-2{\cdot}326\sigma$. If an estimate of this value must be made from 5 results then the value to be used will be $\bar{x}-4{\cdot}40s$, and if 10 results are available the value will be $\bar{x}-3{\cdot}442s$.

If some other value had been chosen as the confidence coefficient, say 99 per cent., the effect would be that the values of k for a sample of 3 would be greatly increased. The increase in the values of k would be less for larger values of n. For very large values of n the choice of confidence coefficient is relatively unimportant, having little effect on the values of k, and in the limit when $n=\infty$ the confidence coefficient has no significance.

3. Size of Sample

The smaller the size of the sample the greater is the uncertainty which must exist in using the results, and the table of values of k shows clearly how increased confidence can be placed in results obtained from larger samples.

From purely theoretical considerations the sample should be as large as possible. From the practical aspect, about 10 results would seem to represent a desirable aim, since this would avoid having to use the very large values of k associated with smaller samples, while larger samples would be unlikely to show an increase in confidence proportional to the cost of the increased testing.

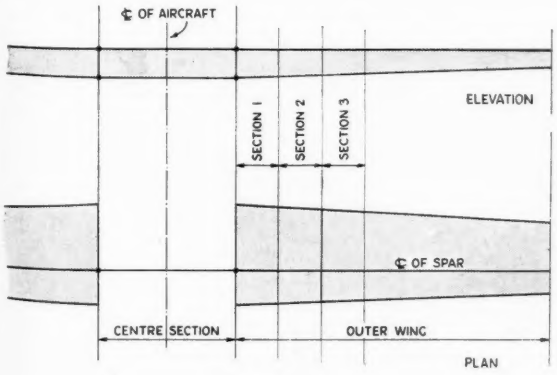

FIGURE 2.

4. Specimen "Life" Calculation

4.1. DESCRIPTION OF WING STRUCTURE

As an illustration of the type of calculations to be made, a wing having a single spar will be considered. The wing will be assumed to consist of a centre section and two outer wings (Fig. 2).

It will be assumed that the most highly stressed portion of the tension boom is that of the centre section, and that sections 1, 2, 3 of the outer wing follow in that order of stress level. Outboard of section 3 it will be assumed that the spar is basically the same as at sections 1, 2, 3 but is more lightly stressed.

4.2. COMPONENTS TO BE TESTED

(*a*) Complete centre section length of tensile boom.
(*b*) Three separate lengths of the outer wing tensile boom corresponding to sections 1, 2, 3.

Note.—In testing the outer wing specimens the test load should be such as to produce the maximum load at the critical section. This may be severe on the other sections in the specimen, but is not likely to have a serious effect if the specimen is not very long.

It will be assumed, for the sake of example, that calculations of the type described in Ref. 1 have shown that sixteen cycles of the test loading correspond to one hour's flying for the aircraft considered (*i.e.* 1 cycle = 0·0625 hours).

4.3. FATIGUE TEST RESULTS

The results shown in Table III are hypothetical but are typical of actual results obtained from specimens representing present-day aircraft. It will be assumed in each case that 7 specimens were tested.

Using these values of $\bar{x}$ and s with the values of k (Table II) corresponding to $n=7$, a range of values has

TABLE III

Test specimen No.	*Reversals to failure*			
	Centre section	*Outer wing Section 1*	*Outer wing Section 2*	*Outer wing Section 3*
1	621,000	923,000	750,000	805,000
2	494,000	778,000	1,254,000	1,250,000
3	362,000	556,000	776,000	1,376,000
4	439,000	478,000	780,000	967,000
5	411,000	623,000	629,000	1,600,000
6	526,000	842,000	830,000	1,342,000
7	390,000	575,000	1,234,000	768,000
Log mean $\bar{x}$	5·65913	5·82288	5·93776	6·04934
s (log basis)	0·08166	0·10554	0·11355	0·12386

been evaluated for each portion of the wing and reversals have been converted to hours (Table IV).

The results of Table IV are of little use in this form since it is the individual probabilities corresponding to a range of values of flying hours for all the sections of the wing that are required. To obtain these probabilities the results of Table IV have been plotted and the derived values are shown in Table V. The following terms have been used:—

p_c = probability of failure of the centre section before a given number N of flying hours.

p_1 = probability of failure of a single outer wing section 1 before N flying hours.

p_2 = probability of failure of a single outer wing section 2 before N flying hours.

p_3 = probability of failure of a single outer wing section 3 before N flying hours.

Since there are two outer wings on the aircraft there are two specimens of each section of outer wing per aircraft. Wing failure will occur before a given number of flying hours N if any one of the centre section, or the two outer wing sections 1, or the two outer wing sections 2, or the two outer wing sections 3, or any portion of outer wing section outboard of section 3, fails before this number of flying hours.

Let

P_{c1} = total probability of failure before N hours of at least one of, the centre section, or the two outer wing sections 1,

P_{c12} = total probability of failure before N hours of at least one of, the centre section, or the two outer wing sections 1, or the two outer wing sections 2,

and so on.

Then

$$P_{c1} = 1 - (1 - p_c)(1 - p_1)^2$$

$$P_{c12} = 1 - (1 - p_c)(1 - p_1)^2 (1 - p_2)^2$$

and if there are m outer wing sections altogether (counted from the centre) the total probability of failure is

$$P_{c12\ldots m} = 1 - (1 - p_c)(1 - p_1)^2 (1 - p_2)^2 \ldots (1 - p_m)^2.$$

When the probabilities to be combined are small

$$P_{c12\ldots m} \simeq p_c + 2p_1 + 2p_2 + 2p_3 \ldots + 2p_m.$$

Using the results of Table V the total probabilities of failure p_c, P_{c1}, P_{c12}, P_{c123} have been calculated, and they are shown in Table VI together with the failure rates associated with the probabilities p_c (centre section alone) and P_{c132} (centre section and two each of outer wing sections 1, 2, 3).

TABLE V

Hours	*Probability of failure*			
	Centre section p_c	*One outer wing section 1* p_1	*One outer wing section 2* p_2	*One outer wing section 3* p_3
25,000	0·45	0·115	0·041	0·0135
22,500	0·28	0·065	0·022	0·0075
20,000	0·15	0·034	0·010	0·0034
17,500	0·063	0·015	0·004	0·0013
15,000	0·02	0·0048	0·0012	0·00041
12,500	0·0033	0·0011	0·0003	0·00011
10,000	0·0003	0·00018	0·00006	0·000025

It will be seen that P_{c123} is not appreciably larger than P_{c12} and so it is reasonable to ignore the outer wing outboard of section 3. (It has already been assumed that the outer wing spar is basically the same design throughout its length and that the stress level is highest at the root and decreases towards the tip).

4.4. LIFE, FAILURE RATE

Hours (Table VI) have been plotted against corresponding failure rate (based on both p_c and P_{c123}) and the result is shown in Fig. 3. It can be seen that a failure rate of 10^{-7} corresponds to a "safe" life of 10,300 hours. If only the centre section had been tested and the life of the whole wing had been based on this one part, the "safe" life would have been 11,300 hours. This in terms of the whole wing corresponds to a failure rate of 2×10^{-7} or twice the accepted risk. In practice, the result of ignoring spar sections other than the portion selected for testing may be much more serious. For example, suppose that for the aircraft considered it was found not possible to test the whole centre section tension boom (where the stress is usually sensibly constant), then it would be wrong to base even the life of the centre section boom on the test results unless an allowance were made for the portion of boom which was not represented in the test specimens.

5. Effect of renewing a part of the structure

It has been shown that, based on the acceptance of one wing fatigue failure in 10^7 flying hours, the "safe" life for the wing tension boom is 10,300 hours. In the

TABLE IV

Probability of failure at life shown p	*Centre section*		*Outer wing Section 1*		*Outer wing Section 2*		*Outer wing Section 3*	
	Reversals	*Hours*	*Reversals*	*Hours*	*Reversals*	*Hours*	*Reversals*	*Hours*
0·5	413,500	25,840	585,850	36,620	755,940	47,250	965,360	60,340
0·1	299,200	18,700	385,520	24,095	481,900	30,120	590,750	36,920
0·01	222,400	13,900	262,730	16,420	318,990	19,940	376,660	23,540
0·002	189,400	11,840	213,530	13,350	255,220	15,950	295,320	18,460
0·001	178,100	11,130	197,080	12,320	234,100	14,630	268,780	16,800
0·0005	167,900	10,500	182,420	11,400	215,770	13,490	245,510	15,340

TABLE VI

Hours	*Combined probabilities of failure*				*Failure rate*	
	Centre section alone	*Centre section +2 outer wing sections 1*	*Centre section +2 outer wing sections 1 + 2 outer wing sections 2*	*Centre section +2 outer wing sections 1 + 2 outer wing sections 2 + 2 outer wing sections 3*	*Based on* p_c	*Based on* P_{c123}
	p_c	P_{c1}	P_{c12}	P_{c123}		
25,000	0·45	0·569	0·604	0·617	$1·80 \times 10^{-5}$	$2·47 \times 10^{-5}$
22,500	0·28	0·37	0·397	0·410	$1·24 \times 10^{-5}$	$1·83 \times 10^{-5}$
20,000	0·15	0·206	0·220	0·230	$7·50 \times 10^{-6}$	$1·15 \times 10^{-5}$
17,500	0·063	0·090	0·097	0·101	$3·60 \times 10^{-6}$	$5·76 \times 10^{-6}$
15,000	0·02	0·0295	0·0319	0·032	$1·33 \times 10^{-6}$	$2·14 \times 10^{-6}$
12,500	0·0033	0·0054	0·0060	0·006	$2·64 \times 10^{-7}$	$4·80 \times 10^{-7}$
10,000	0·0003	0·00067	0·00079	0·0008	$3·00 \times 10^{-8}$	$8·00 \times 10^{-8}$

example taken, the centre section and outer wings have lives of similar order and so in this case there is no alternative to renewal of centre section and outer wing tension booms. It could happen that the centre section has a much shorter life than the outer wings. In this case replacement of the centre section would be all that is required, provided that new life calculations are performed combining the probabilities relevant to a new centre section with the probabilities of the outer wings, which have already flown as many hours as the old centre section. At no time should the failure rate be allowed to exceed 1 in 10^7 hours, or whatever is the chosen value.

6. Effect of Type of Structure

The previous calculations dealt with a wing having a single spar. With a two-spar wing, since failure of either spar could lead to complete wing collapse, specimens representing portions of both spars should be tested and the overall life obtained in a similar manner to that used for dealing with the individual portions of the single spar.

The determination of the "safe" life of a multi-spar box wing structure is more difficult, since in this case it may be possible for some structural members to fail without subsequent immediate failure of the wing. The danger in relying on this fact is that failures of this kind may not be discovered until long after they have occurred. In the intervening period the wing will have a reduced static strength and the consequent rate of fatigue damage to the remaining structure will be increased. For this reason, it is suggested that a multi-spar box wing which has heavy spar booms should be given the same life as a similar wing having spars only, in which the spar stresses are the same.

It is not possible to generalise on multi-spar structures in which the tensile load is well distributed between spar booms, skin, and stringers and in which the spar booms are light members. If it is wished to take advantage of the distribution of the load to increase the "safe" life, then it would be necessary to test a specimen structure so that the largest tension member is made to fail and it is then demonstrated that the remaining structure has an adequate static strength and fatigue life.

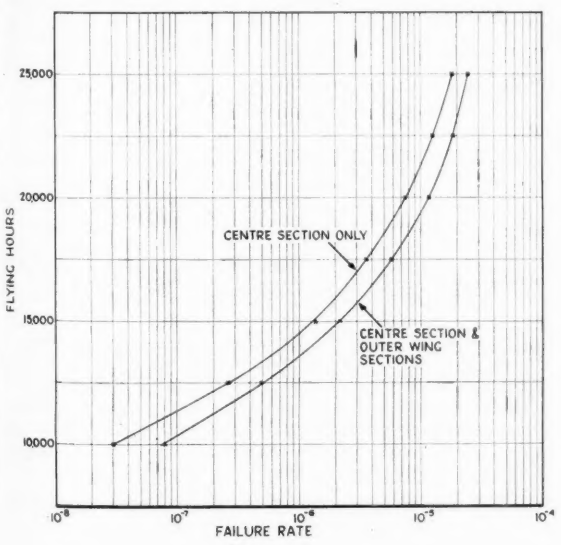

FIGURE 3.

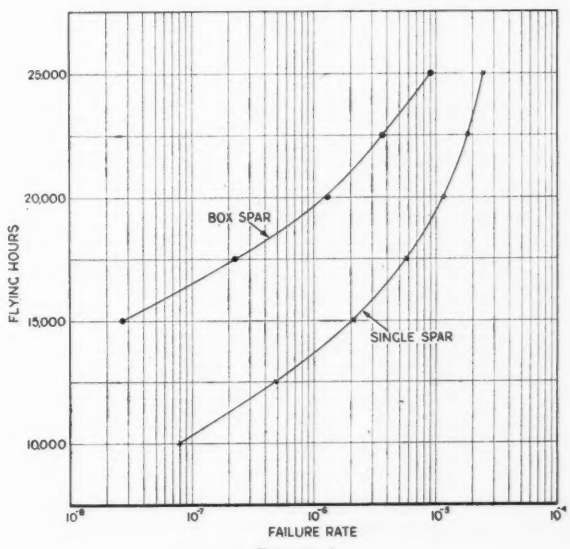

FIGURE 4.

TABLE VII

Hours	*Combined probabilities of failure*				*Failure rate*
	Centre section alone	*Centre section + 2 outer wing sections 1*	*Centre section + 2 outer wing sections 1 + 2 outer wing sections 2*	*Centre section + 2 outer wing sections 1 + 2 outer wing sections 2 + 2 outer wing sections 3*	*Based on* P_{c123}
	P_c	P_{c1}	P_{c12}	P_{c123}	
25,000	0·2025	0·222	0·226	0·228	$9{\cdot}10\times10^{-6}$
22,500	0·0784	0·084	0·085	0·085	$3{\cdot}78\times10^{-6}$
20,000	0·0225	0·026	0·026	0·026	$1{\cdot}30\times10^{-6}$
17,500	0·0040	0·004	0·004	0·004	$2{\cdot}29\times10^{-7}$
15,000	0·0004	0·0004	0·0004	0·0004	$2{\cdot}67\times10^{-8}$

7. Specimen "Life" Calculations—Two-spar Box Structure

A two-spar box wing structure having light spars will now be considered. It is assumed that the box is continuous from tip to tip, and that tests have shown that failure of one spar boom would still leave the wing with an adequate reserve of strength, but that failure of both spar booms in or near the same chordwise section would seriously weaken the structure. It will be further assumed that fatigue test results are available for typical members used to make the box tension booms and that, section by section, the results are the same as for the boom of the single spar previously considered.

The criterion for failure is that, in or near the same chordwise section, both front and rear spar booms should fail before a certain number of flying hours. Then the life of the box structure can be obtained in the following manner.

Let P_c = probability of failure before a certain number of flying hours of both front and rear spar booms in the centre section.

Then

$$P_c = 1-(1-p_c^2)$$

$$P_{c1} = 1-(1-p_c^2)(1-p_1^2)^2$$

$$P_{c12} = 1-(1-p_c^2)(1-p_1^2)^2(1-p_2^2)^2$$

$$P_{c13} = 1-(1-p_c^2)(1-p_1^2)^2(1-p_2^2)^2(1-p_3^2)^2.$$

When the probabilities to be combined are small,

$$P_{c123} \simeq p_c^2+2p_1^2+2p_2^2+2p_3^2.$$

The combined probabilities are given in Table VII.

It can be seen that for a failure rate of 10^{-7} the "safe" life for the box spar is 16,500 hours, which is an increase of about 60 per cent. on the corresponding value for the single spar wing (Fig. 4)

8. Conclusions

It has been suggested that the distribution of reversals to failure obtained from fatigue tests on specimens typical of aircraft wing booms can be represented by a log-normal curve. A method, based on the acceptance of a certain fatigue failure rate for wing structures, has been given for calculating the number of reversals to failure on which the "safe" life of a wing may be based from results obtained from small samples. Since very small samples involve penalties in the form of large factors to allow for the consequent unreliability of the results, and large samples are costly, a sample size of 10 has been suggested as a suitable compromise. It has been emphasised that it is not sufficient to test one section of a spar boom, even if it is the most highly stressed section. Only those portions of the spar boom which are so lightly stressed that the probability of their failure is negligible, when compared with the summation of the probabilities of failure of the more highly stressed sections of the spar boom, may be omitted. Attention has been drawn to the fact that when dealing with a highly stressed section of the spar boom the full length of the section must be tested or else an allowance must be made for the length of boom which was not represented in the test specimens. Finally, it has been suggested that some box structures would appear to offer a greater "safe" life than heavy spar structures which are working at the same stress level.

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TECHNICAL NOTES

Contributions to this Section of the JOURNAL *will be eligible for Journal Premium Awards and will normally be published within two months of being received.*

Some Notes on the Torsion-Bending Constant

by

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THE TORSION-BENDING CONSTANT of open sections is of great importance in the calculation of buckling loads under compression of struts failing in the torsional or torsional-flexural mode. It involves computation of the warping of the cross section and misunderstandings arising out of terminology and notation often mislead the users of the formulae. The purpose of this note is to clarify the terminology without going into proofs as well as presenting the equations required using a consistent notation.

NOTATION

A area of cross section of the strut (in.^{2})
E Young's modulus (lb./in.^{2})
G shear modulus (lb./in.^{2})
I_{xx} moment of inertia of the cross section about axis x (in.^{4})
I_{yy} moment of inertia of the cross section about axis y (in.^{4})
I_{xy} product of inertia of the cross section about axes x, y (in.^{4})
(Similarly for axes ξ, η)
J torsion constant (GJ = torsional rigidity) (in.^{4})
m_ξ moment of the cross section area about axis ξ (in.^{3})
m_η moment of the cross section area about axis η (in.^{3})
R_ξ, R_η *see* equations (27) (in.^{5})
r_t distance from the origin of co-ordinates to the tangent to the middle line (*see also* superscripts); without superscript refers to the centroid C (in.)
T applied torque (lb. in.)
t wall thickness of the cross section (in.)
U_l length of middle line at the "lower" end of the cross section: either negative or zero (in.)
U_u length of the middle line at the "upper" end of the cross section, always positive (in.)
u linear co-ordinate of the middle line measured from $u=0$ in the positive direction towards U_u (in.)
W warping of the cross section (in.)
w unit warping of cross section (*see* equation (6)) (in.^{2})
w_0 *see* equation (6) (in.^{2})
w_u *see* equation (7) (in.^{2})
x, y rectangular co-ordinates of the cross section, with the centroid C as origin (in.)
x^*, y^* rectangular co-ordinates of the cross section, with the centre of shear S as origin (in.)
$\bar{x}, \bar{y}$ rectangular co-ordinates of the cross section, with the centre of rotation T as origin (in.)
z co-ordinate perpendicular to the x, y plane, parallel to the length of the strut (in.)
Γ torsion-bending constant (*see* superscripts) (in.^{6})
ξ, η as x, y with an arbitrary point O as origin (in.)
ϕ angle of twist (rad.)

Geometric Notations

C centroid
O any point of reference
S centre of shear
T centre of rotation

Superscripts

* referring to S
$^+$ referring to O
$^-$ referring to T

Note: when no superscript is shown, values refer to the centroid C.

WARPING

The warping of any cross section, at a point u on the middle line of the cross section is obtained (provided the cross section does not distort) by equating to zero the expression for shear strain of an element formed by the generator and the middle line, which must be zero. If the warping is designated as W (in.) then (*see* Fig. 1)

$$\frac{\partial (r_t \phi)}{\partial z} + \frac{\partial W}{\partial u} = 0. \quad . \quad . \quad (1)$$

As r_t is a constant independent of z (the strut being of uniform cross section), (1) can be re-written (using $\partial\phi/\partial z = \phi'$) in the form

$$\frac{\partial W}{\partial u} = -r_t \phi' \quad . \quad . \quad . \quad (2)$$

and hence, since ϕ' is independent of u,

$$W = -\phi' \int r_t du + f(z). \quad . \quad . \quad (3)$$

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Two cases must be considered:

Case 1. $\phi' =$ constant, *i.e.* Saint-Venant's condition for torsion. This means that the twist is uniform and is independent of z, and

$$f(z)=0. \quad . \quad . \quad . \quad (4)$$

Equation (3) then becomes

$$W=-\phi' \int r_t du. \quad . \quad . \quad (5)$$

The integral is then re-written as

$$\int r_t du = w_0 + \int_0^u r_t du = w \quad . \quad . \quad (6)$$

and finally

$$W=-\phi' w. \quad . \quad . \quad . \quad (5a)$$

w is the unit warping, since it is the warping per unit of twist, and has the dimension of an area.

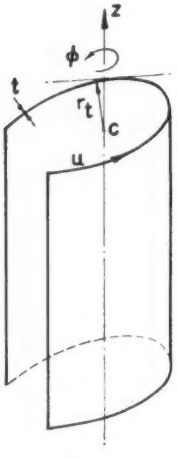


FIGURE 1.

The zero position of u can be chosen at random, and it will affect the value of the integral expression

$$w_u = \int_0^u r_t du. \quad . \quad . \quad (7)$$

The final expression for unit warping is then

$$w = w_0 + w_u, \quad . \quad . \quad . \quad (8)$$

of which w_0 is a constant (to be dealt with later) and w_u a variable depending both on the point of origin O and the point of reference u. It is important to bear in mind that at the origin $u=0$, w_u is also equal to zero†. The point that $(w_u)_0=0$ is illustrated in Fig. 2. As the sign convention for u is arbitrary, a sign convention for r_t is needed. The simplest one is the sign convention suggested by Levy and Kroll††; if r_t is rotated by 90° clockwise, in order to coincide with the positive direction of the tangent to the middle line, then r_t is taken as negative; if a rotation of 90° is anti-clockwise, r_t is positive (*see* the example of Fig. 2).

The value of w_0 is at this stage arbitrary, in fact changing w_0 means changing the reference plane from which W is measured. This has no effect on the stress distribution, which depends on the variation of W along u.

Case 2. $\phi' \neq$ constant, *i.e.* the twist is not uniform along the length. This may be due either to conditions restraining the warping at some section (usually one of the ends), or to variation of torque applied along the length of the strut z. In both cases if no special restraints are imposed, the rotation will take place about the centre of shear S (*see* Fig. 3). Denoting all values referred to the centre of shear S by the superscript*,

$$W^* = -\phi' w^* + f(z), \quad . \quad . \quad (9)$$

and

$$w^* = w_0^* + \int_0^u r_t^* du = w_0^* + w_u^*. \quad . \quad (10)$$

Conditions for no resultant loads in the z-direction as well as no moments about the x^* and y^* axes yield the following expression

$$W^* = -\phi' w^*, \quad . \quad . \quad . \quad (9a)$$

provided that

$$\int_A w^* dA = 0. \quad . \quad . \quad . \quad (11)$$

In expression (11), $dA = t\,du$ is the area element, and the integration extends over the entire area of the cross section; (11) can be re-written as

$$\int_{U_l}^{U_u} w^* t\, du = 0. \quad . \quad . \quad . \quad (11a)$$

where U_l and U_u are the values of u at the ends of the middle line (*see* Fig. 3). U_l is either negative or zero, U_u is positive. Condition (11) provides for the computation of w^* values, since it gives a definite value for w_0^*:—

$$w_0^* = -\frac{1}{A} \int_{U_l}^{U_u} w_u^* t\, du. \quad . \quad . \quad (12)$$

Finally

$$w^* = -\frac{1}{A} \int_{U_l}^{U_u} w_u^* t\, du + w_u^*. \quad . \quad . \quad (13)$$

Note as before that w_u^* depends on the zero position of u as does w_0^*, but w^* is independent of the choice of the point of origin on the middle line.

If the axis of rotation is prescribed and does not pass through the centre of shear of the cross section, the foregoing relations will become ($\overline{W}$, etc., referring to any axis of rotation)†

$$\overline{W} = -\phi' \overline{w} \quad . \quad . \quad . \quad (14)$$

†Thus for a negative value of $u=-a$, $w_u = \int_0^{-a} r_t du$.

††*Journal of the Aeronautical Sciences*, p. 583, October 1948.

†One should be careful to distinguish between O, the point of reference, and T, the centre of rotation forced upon the strut.

where $$\overline{w}=\overline{w}_0+\int_0^u \bar{r}_t\,du=\overline{w}_0+\overline{w}_u, \quad . \quad . \quad (15)$$

provided that $$\int_{U_l}^{U_u} \overline{w}_u t\,du=0. \quad . \quad . \quad . \quad (16)$$

Hence $$\overline{w}=-\frac{1}{A}\int_{U_l}^{U_u} \overline{w}_u t\,du+\overline{w}_u. \quad . \quad . \quad (17)$$

It is important to bear in mind that equations (11*a*) and (16) apply to axes of rotation only, *i.e.* S the shear centre if the strut is subjected to conditions referred to in the case of $\phi' \neq$ constant, or T the actually prescribed axis. It does not apply automatically to either the axis through the centroid C nor to any "reference" axis such as O on Fig. 3.

TORSION-BENDING CONSTANT

The effect of warping on torsional stiffness is best expressed by the equation

$$\frac{dT}{dz}=GJ\phi''-\overline{\Gamma}E\phi^{iv}, \quad . \quad . \quad (18)$$

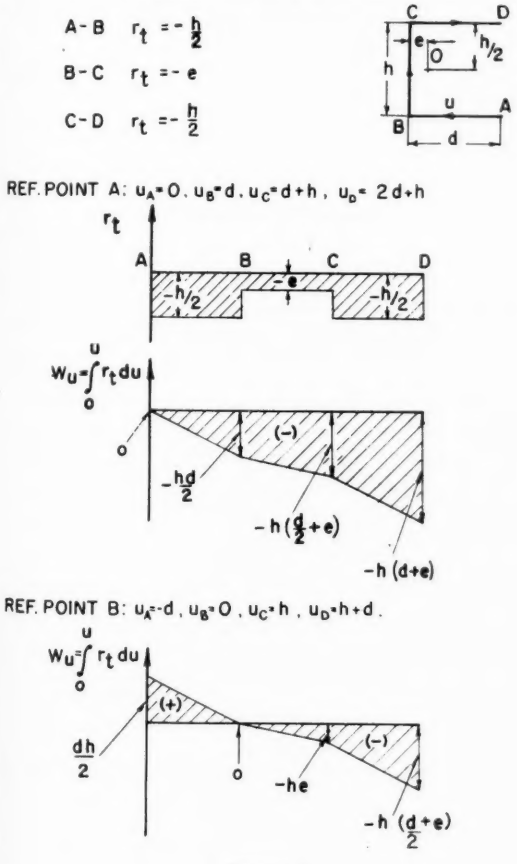
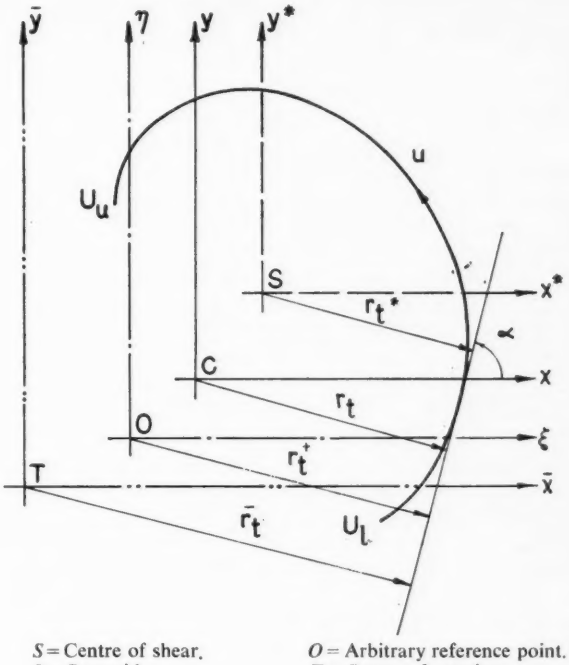


FIGURE 2.



S = Centre of shear. O = Arbitrary reference point.
C = Centroid. T = Centre of rotation.

FIGURE 3.

where T is the torsional moment applied, and $\overline{\Gamma}$ the torsion-bending constant referred to the axis of rotation (which may or may not be the axis through the centre of shear).

Actually $\overline{\Gamma}=\overline{\Gamma}_1+\overline{\Gamma}_2$, where $\overline{\Gamma}_1$ relates to the direct strain along the middle line, *i.e.* in the middle of the wall, whereas $\overline{\Gamma}_2$ relates to the same strain across the thickness of the wall. $\overline{\Gamma}_2$ is actually very small in comparison with $\overline{\Gamma}_1$ and is usually neglected, except when the axis of rotation is so placed that $\bar{r}_t=0$ for every point on the periphery. In that case the value of $\overline{\Gamma}$ is that of $\overline{\Gamma}_2$. Only $\overline{\Gamma}_1$ will be dealt with here.

As the prescribed axis of rotation can be anywhere, very general types of expressions for $\overline{\Gamma}_1$ would be needed. To simplify this, $\overline{\Gamma}_1$ values in all textbook and data sheets (*i.e.* Γ_1^*) are referred to the centre of shear S. In the most general form

$$\overline{\Gamma}_1=\int_A \overline{w}^2 dA=\int_{U_l}^{U_u} \overline{w}^2 t\,du. \quad . \quad (19)$$

If the axis of rotation passes through the centre of shear

$$\Gamma_1^*=\int_A w^{*2} dA=\int_{U_l}^{U_u} w^{*2} t\,du. \quad . \quad (20)$$

Replacing w^* and $\overline{w}$ by their expressions (13) and (17) and simplifying, gives

$$\overline{\Gamma}_1=\int_{U_l}^{U_u} \overline{w}_u^2 t\,du-\frac{1}{A}\left(\int_{U_l}^{U_u} \overline{w}_u t\,du\right)^2, \quad . \quad (19a)$$

$$\Gamma_1^* = \int_{U_l}^{U_u} w_u^{*2} t\, du - \frac{1}{A}\left(\int_{U_l}^{U_u} w_u^* t\, du\right)^2 . \qquad (20a)$$

There are three ways for computing $\overline{\Gamma}_1$:—

(*i*) To use the direct formule (19*a*), *i.e.* to compute the warping with reference to axis of rotation T and substitute it in equation (19*a*). This is usually done in the case of unusual cross sections, or cross sections for which no Γ_1^* (referred to the centre of shear S) values or any other Γ_1 values have been computed.

(*ii*) When the value Γ_1^* is known, to use transformation formulae connecting $\overline{\Gamma}_1$ with Γ_1^*. If $\bar{r}_t$ is replaced by (*see* Fig. 3)

$$\bar{r}_t = r_t^* + \bar{x}_S \sin\alpha - \bar{y}_S \cos\alpha = r_t^* - x_T^* \sin\alpha + y_T^* \cos\alpha, \qquad (21)$$

where $\cos\alpha = dx/du$ and $\sin\alpha = dy/du$, and substituted in (19*a*), the following expression is obtained:—

$$\overline{\Gamma}_1 = \Gamma_1^* + I_{xx} x_T^{*2} + I_{yy} y_T^{*2} - 2I_{xy} x_T^* y_T^* . \qquad (22)$$

Since $\bar{x}_S = -x_T^*$ and $\bar{y}_S = -y_T^*$, either can be used in (22) without change of signs. I_{xx}, I_{yy} and I_{xy} are the moments of inertia and the product of inertia of the cross section area referred to the centroid C; whereas x_T^*, y_T^* (respectively $\bar{x}_S$, $\bar{y}_S$) are the co-ordinates of the centre of rotation T with respect to the centre of shear S (it is understood that the directions of the systems of rectangular co-ordinates are arbitrarily chosen, but are all parallel to each other).

(*iii*) When the value of Γ_1^* and/or the position of S is not known, also when the position of T is varying, so that several computations—or a complete analysis of all possible positions—is required, Γ_1^+ referred to some particular point of reference O may be of great help, particularly if point O is made to coincide with the position of $u = 0$. If $\bar{r}_t$ is replaced by (*see* Fig. 3)

$$\bar{r}_t = r_t^+ + \bar{x}_0 \sin\alpha - \bar{y}_0 \cos\alpha, \qquad (23)$$

where $\cos\alpha = d\xi/du$ and $\sin\alpha = d\eta/du$, and substituted in the expression for $\overline{w}_u$, the following equation is obtained

$$\overline{w}_u = \int_0^u \bar{r}_t du = \int_0^u r_t^+ du + \bar{x}_0(\eta - \eta_0) - \bar{y}_0(\xi - \xi_0). \qquad (24)$$

Denoting $\int_0^u r_t^+ du = w_u^+$, (25)

and substituting it in equation (19*a*), the following expression is obtained

$$\overline{\Gamma}_1 = \int_{U_l}^{U_u} (w_u^+)^2 t\, du - \frac{1}{A}\left(\int_{U_l}^{U_u} w_u^+ t\, du\right)^2 + \bar{x}_0^2\left(I_{\xi\xi} - \frac{m_\xi^2}{A}\right) + \bar{y}_0^2\left(I_{\eta\eta} - \frac{m_\eta^2}{A}\right) - 2\bar{x}_0\bar{y}_0\left(I_{\xi\eta} - \frac{m_\xi m_\eta}{A}\right) + 2\bar{x}_0\left[\int_{U_l}^{U_u} w_u^+ \eta t\, du - \frac{m_\xi}{A}\int_{U_l}^{U_u} w_u^+ t\, du\right] - 2\bar{y}_0\left[\int_{U_l}^{U_u} w_u^+ \xi t\, du - \frac{m_\eta}{A}\int_{U_l}^{U_u} w_u^+ t\, du\right]. \qquad (26)$$

If

$$\left.\begin{aligned} \Gamma_1^+ &= \int_{U_l}^{U_u} w_u^{+2} t\, du - \frac{1}{A}\left(\int_{U_l}^{U_u} w_u^+ t\, du\right)^2; \\ R_\xi &= \int_{U_l}^{U_u} w_u^+ \eta t\, du - \frac{m_\xi}{A}\int_{U_l}^{U_u} w_u^+ t\, du; \\ R_\eta &= \int_{U_l}^{U_u} w_u^+ \xi t\, du - \frac{m_\eta}{A}\int_{U_l}^{U_u} w_u^+ t\, du; \end{aligned}\right\} \qquad (27)$$

and noting that

$$\left.\begin{aligned} I_{\xi\xi} - \frac{m_\xi^2}{A} &= I_{xx} \\ I_{\eta\eta} - \frac{m_\eta^2}{A} &= I_{yy} \\ I_{\xi\eta} - \frac{m_\xi m_\eta}{A} &= I_{xy} \end{aligned}\right\} \qquad (28)$$

the final expression for $\overline{\Gamma}_1$ becomes

$$\overline{\Gamma}_1 = \Gamma_1^+ + \bar{x}_0^2 I_{xx} + \bar{y}_0^2 I_{yy} - 2\bar{x}_0\bar{y}_0 I_{xy} + 2\bar{x}_0 R_\xi - 2\bar{y}_0 R_\xi . \qquad (29)$$

Equation (29) is very convenient when the computation of the position of the centre of shear is to be avoided; also when one of the values of $\bar{x}_0$ or $\bar{y}_0$ can be reduced to zero by a proper choice of the point O.

Definition of Specific Impulse

by

K. ASQUITH, B.A., A.F.R.Ae.S., and J. G. WHITAKER, B.Sc., Grad.R.Ae.S.

TWO RECENT CONTRIBUTORS to Technical Notes have expressed the view that, in rocketry, specific impulse should be defined as thrust/*mass* of propellants consumed per second, rather than as thrust/*weight* of propellants consumed per second. The only reason given in support of this view is that the weight of a given mass varies with location. Our purpose here is to give reasons for taking the opposite view, namely that the second definition is preferable.

The impulse given to a rocket by the burning of its propellants is $\int_0^t P\, dt$, where P is the thrust at time t and

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T is the time of burning. Thus, logical alternative definitions of specific impulse are

$$(\text{S.I.})_m = \frac{\int_0^T P\,dt}{m} \quad ; \quad (\text{S.I.})_w = \frac{\int_0^T P\,dt}{w}$$

where m and w are respectively the mass and weight of propellants consumed. If rocket thrust and propellant flow are both constant, the above formulae simplify to

$$(\text{S.I.})_m = P/\dot{m} \quad ; \quad (\text{S.I.})_w = P/\dot{w}.$$

For convenience, these will be called the "mass" and "weight" definitions respectively. We note that $(\text{S.I.})_m$ has the dimensions of a velocity, whereas $(\text{S.I.})_w$ has the dimensions of time.

An important advantage of the weight definition is that it gives values of specific impulse which are independent of the units employed, provided only that the two forces involved (thrust and weight) are measured in the same units (for example, both in poundals, or both in pounds weight). The mass definition, on the other hand, gives a value of specific impulse which varies according to the choice of units.

Consider, for example, a rocket of thrust 2,000 pounds weight and rate of mass consumption of propellants 10 pounds per second. Since one pound mass weighs one pound weight, the rate of weight consumption of propellants is 10 pounds weight per second. We thus have, using gravitational units,

$$(\text{S.I.})_w = \frac{2{,}000}{10}\,\frac{\text{lb. wt.}}{\text{lb. wt. per sec.}} = 200 \text{ sec.}$$

and if we use absolute units

$$(\text{S.I.})_w = \frac{2{,}000 \times 32{\cdot}2}{10 \times 32{\cdot}2}\,\frac{\text{pdl.}}{\text{pdl. per sec.}} = 200 \text{ sec.}$$

On the other hand, if we use gravitational units

$$(\text{S.I.})_m = \frac{2{,}000}{10}\,\frac{\text{lb. wt.}}{\text{lb. per sec.}} = 200 \text{ units}$$

whereas if we use absolute units

$$(\text{S.I.})_m = \frac{2{,}000 \times 32{\cdot}2}{10}\,\frac{\text{pdl.}}{\text{lb. per sec.}} = 6{,}440 \text{ ft. per sec.}$$

Thus confusion both as to numerical value and as to units is much more likely to arise if the mass definition rather than the weight definition is employed.

Another strong point in favour of the weight definition is that, in most of the existing literature, specific impulse is expressed in seconds. If, as Professor Thorne implies, this has arisen from inability to distinguish between mass and weight, then the present adoption of the weight definition will remove any stigma of illegitimacy, and rocket engineers can continue in the use of units with which they are familiar.

Further, it may be noted that the mass definition, when consistent units (poundals and pounds, or pounds weights and slugs) are used, gives us the "effective efflux velocity," a term already defined in the basic equation for rocket thrust: to re-define this as specific impulse is both unnecessary and confusing.

It is admitted that $(\text{S.I.})_w$ varies with location, whereas, provided absolute units are used, $(\text{S.I.})_m$ does not. But most advocates of the mass definition (including, apparently, Mr. Greenwood) use gravitational units in the numerator: thus their $(\text{S.I.})_m$ varies with location —the very criticism that was levelled against $(\text{S.I.})_w$!

The Comparison of a "Wake Brake" and a Parachute for the Landing of Aeroplanes

by

S. B. JACKSON

(Chief Aerodynamicist, Irving Air Chute of Great Britain Ltd.)

IT WOULD SEEM that Mr. Heughan (JOURNAL, March 1954) has missed the basic design criterion of drag exerted for a given weight (and bulk) penalty. No mention is made of this fundamental point in the choice of a device for the reduction of the landing run of an aircraft. The choice of comparison of the "Wake brake" with a 7 ft. brake parachute has therefore been made somewhat arbitrarily. Brown does not state that "the *usual* size of a parachute used for braking an aeroplane is 7 ft."; on page 271 of the reference concerned ("Parachutes," Sir Isaac Pitman, 1951), a case for "a particular aircraft" was quoted as "using a 7 ft. parachute." The aircraft concerned was not the heavily loaded present-day jet fighter with appropriate approach speeds. If, say, the 16 ft. flat parachute quoted by Mr. Heughan for the F-94C Starfire is compared instead with a drag of some 2·3 times that of the 7 ft. one, does the "Wake brake" then appear so favourable? The weight and bulk of an equivalent British design to the Lockheed parachutes are 12 lb. and 1,000 cu. in. respectively, this parachute being stressed for landings up to an aircraft speed of 200 knots.

Apart from what is felt to be a misconception in outlook, there is a most serious error in paragraph 3 on page 201 of Mr. Heughan's Note. It is there stated that the "tail buffeting is caused by the use of a parachute downstream of the tail," Brown's book again being

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quoted as the reference. In fact, on page 270 of Brown's book, the reverse is stated, buffeting being sometimes attributable to air brakes and *avoidable* by the use of a brake parachute. Mr. Heughan's second point in this paragraph, concerning disaster from a misjudged landing approach, may also be avoided in future without the somewhat impracticable solution of using a spare parachute.

D. M. HEUGHAN

I WISH TO THANK Mr. Jackson for his comments on my Note, particularly so as they come from one interested in the use of a brake parachute for the landing of aeroplanes.

The main purpose of the Note was to compare the drag of a parachute with that of a "Wake brake" scheme. The fact that it is easier to add a tail parachute container to an aeroplane than it is to incorporate the complicated rear suction slots, ducts and valve system required for the "Wake brake," is obvious and was therefore, not stressed in the Note.

It appears that Mr. Jackson did not notice either the example of a 10 ft. diameter parachute or the assumed drag coefficient of a parachute of 1·50 in the Note. Consequently, his ratio of 2·3 comparing the drags of the American 16 ft. diameter parachute and the assumed 7 ft. diameter parachute is a little on the generous side, to his advantage. The ratio of the drags of the 16 ft. and 7 ft. diameter parachutes is, at a given speed, the ratio of the $(C_{Dp} \times d^2)$ values or

$$\frac{16^2 \times 0{\cdot}49}{7^2 \times 1{\cdot}50} = 1{\cdot}71.$$

The ratio of 2·3 quoted by Mr. Jackson is derived presumably by taking a drag coefficient of about 1·12 for the 7 ft. diameter parachute. This value may well be correct in practice but it gives the impression that the drag of the American 16 ft. diameter parachute is greater than that of a 10 ft. diameter parachute (or 2 of 7 ft. diameter) of drag coefficient 1·50, which is not so. Thus, the example quoted by Mr. Jackson has been more than covered by the cases evaluated in the Note and further, explains why the 16 ft. diameter parachute was not discussed further in the Note in comparing its drag with that of a "Wake brake" scheme.

Mr. Jackson quotes the weight of a British parachute equivalent to the 16 ft. diameter Lockheed parachute as 12 lb. This presumably is the weight of the parachute alone as the weight of the whole American assembly is reported to be 65 lb. Although the total weight is still small the figure quoted by Mr. Jackson could be misleading.

Mr. Jackson is correct in pointing out the error with respect to tail buffeting which can be a feature of air brakes as he states. It was intended to convey in the Note that lateral instabilities are sometimes experienced with certain parachutes and the mistake is regretted.

The addition of a spare parachute to overcome a misjudged landing approach is of course sound. In addition, the spare parachute may be of value if there was any mishap with the first one during the landing run, as for example due to the action of a side gust. It was in fact reported recently that a Vulcan bomber had its tail parachute draped over a wing due to such an occurrence (*Flight*, 19th March, 1954, p. 327).

G. L. GUNSTONE, A.F.R.Ae.S.

MR. HEUGHAN proposes an aircraft braking system, the operation of which involves the bleeding away from a wing slot of some hundred pounds of air per second. This he proposes to do by connecting the bleed to the intake of the gas turbine engine(s).

From the details of the aeroplane given in the Note (*e.g.* the wing span) it is to be assumed that the engine he has in mind is a conventional gas turbine of some 5-8,000 lb. thrust, and it must also be assumed therefore that the air mass flow of 110 pounds per second represents the *take-off* condition. The author has therefore introduced a wake drag only at the expense of maintaining take-off thrusts under landing conditions. When comparing the usefulness of this concept with a tail parachute, therefore, this difference between the normal engine landing thrust (500 lb. approximately) and the take-off thrust (say, 5,500 lb.) must be deducted from the drag obtained from the "Wake brake." This leads, of course, to an absurd condition.

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D. M. HEUGHAN

THE ASSUMPTION of an air flow of 110 lb./sec. of course refers to the landing condition and not to the take-off condition as Mr. Gunstone assumes. A normal landing is envisaged as far as thrust is concerned. However, whether these requirements would need a special gas turbine is something which would have to be decided with the gas turbine manufacturers, if the scheme were to be considered in more detail. It is appreciated that the large air flow associated with the "Wake brake" is a distinct disadvantage, as discussed in my Note, but clearly, the more air withdrawn through the rear suction slots the better from the point of view of the wake drag.

There are two other factors, both of which reduce thrust, to be elucidated before the specification of a suitable gas turbine can be drawn up. These are namely, the loss of the ram effect with the use of rear suction slots and the possibility of surging of the compressor with the change over from forward to rear intakes.

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Reviews

FIRST THROUGH THE CLOUDS. *F. Warren Merriam. Batsford 1954. 176 pp. Photographs. 21s. net.*

This is an enjoyable presentation of early flying days. Merriam did not take part actually in the birth of flight, but he took a prominent part in the weaning..

His keenness to learn and his first efforts are well and engagingly told. Later he passes from the instructed to being an instructor, and here in this hazardous occupation, there pass through his hands some very well known people.

Considering the cranky machines, their unreliable engines, the lack of proper installations for dual control, and the occasional ham handed pupils he had to contend with, the fact that he is alive today is nothing short of remarkable.

I very much appreciate his charitable approach to the early pioneers. He speaks with enthusiasm and admiration of people like the Short Brothers, Tommy Sopwith, A. V. Roe, etc. That is as it should be for they were great people.

Early flying in this country was divided broadly into three camps, if I may so describe them. There was, first of all, Leysdown, later Eastchurch in the Isle of Sheppey, then Brooklands and finally Hendon. Each was a world of endeavour and experiment in itself, and it is of Brooklands that this book deals, and very brightly and entertainingly is the story told.

Although I built a machine in 1907 at Brooklands side by side with A. V. Roe, the machine never looked like flying, nor did it, but at that time we little visualised that not on the track but in the middle of this ground would there soon be an aerodrome busy with many pioneers fitting on their early wings.

Brooklands will always stand out as an aerodrome where much early activity took place, and although I never took part in those efforts, it was a cheery intensive hub of early aviation.

The actual title of " First through the Clouds " is a claim, which may or may not be disputed, but the description of the flight is good, and not to be belittled, as awareness of balance in clouds completely vanishes.

As an Instructor Merriam had a remarkable career. He instructed such men as Brancker, Dowding, Lankester Parker, W. Salmon, Alcock, Longcroft and Beck, and among others he got Pemberton Billing his ticket in one day for a bet, but only just!! What a record to be proud of.

He tells us that out of the ninety pilots who took the four R.F.C. Squadrons, numbers 2, 3, 4 and 5 across the Channel in 1914, he had instructed twenty-nine of the pilots! And what a grand crowd they were. They shot at Germans with revolvers at long range with little success, but on the other hand brought back most valuable reconnaissance information, which was never believed.

This man had bad eyesight yet the way he struggled and wangled everyone to allow him to fly is a lesson in aggressive refusal to accept the opinion of the expert.

Transferred to the R.N.A.S. his work at Chingford merits close attention, for it is full of human interest, and great accomplishment.

During the First World War he sank a submarine, a very remarkable achievement, as tools then were not very efficient.

He tells us he flew up to the second war, 5,000 hours and had flown sixty types. I never knew there were so many. What a man!

His struggle to earn a living after all his heroic work in building up British aviation is sad reading, as also is the inevitable " write off " of machines and men.

I commend this book to all who would like some of the heroic days recalled, but I commend it also as the story of a brave man, inspired with the love of flying, and accomplishing, year after year, work for which all are in his debt, but from whom he has received few " Thank you's."

From the bottom of my heart let me here and now tell him what a noble job he did in his hard working wonderful career, coupled with the hope that this inspiring book will be the success it deserves.—BRABAZON OF TARA.

INTRODUCTION TO AERONAUTICAL DYNAMICS. *Manfred Rauscher. Chapman & Hall 1953. 664 pp. Index. 96s. net.*

This is an unusual book. Half of it gives an account of material which is usually treated as dynamics and generally learnt by students before the applications to aeronautics are demonstrated. The other half is a straightforward treatment of the elements of fluid mechanics. The author states that the book has been written over the past twenty years and that it has grown out of the courses in aeronautics given by him to the " junior year " at the Massachusetts Institute of Technology. Exceptional trouble has been taken to explain all difficult concepts as clearly as possible and no short cuts are taken anywhere; this partly explains the length of the work. The author has avoided the use of vector analysis, considering that this is too difficult for the level of the students concerned. Analysis of aircraft stability, which might be considered to be the principal subject of aeronautical dynamics, is not treated. The groundwork for study of it is given but the theory of aircraft stability as such is left to be learnt at a later stage.

A list of chapter headings indicates the ground covered:

(1) Kinematics of a point, (2) Dynamics of a particle, (3) Dynamics of groups of particles, (4) Fundamental equations of fluid motion, (5) Stream function and velocity potential, (6) Fluid motion about simple bodies, (7) Transformations, (8) Aerofoils, (9) Aerofoils in three dimensions, (10) Viscosity, (11) Kinematics of rigid bodies, (12) Dynamics of rigid bodies, (13) Oscillations of systems with one degree of freedom, (14) Oscillations of systems with more than one degree of freedom.

A large number of illustrative exercises is given at the end of each chapter.

Standard methods are used in the chapters on fluid mechanics. The treatment of flow over aerofoils by conformal transformation is taken to a fairly advanced level, in fact farther than is really needed by undergraduate students. The inclusion of this section may be associated with the long time for which the book has been in preparation, as the importance of conformal transformation as a method of aerofoil design has diminished in the past few years, except for specialist applications. In a later edition part of this chapter might be replaced by the more useful treatment of thin aerofoils.

The lifting line theory for wings of finite span is presented on the same lines as in Glauert's book, but in more detail. Dr. Rauscher is the first to publish an

elementary derivation of the Biot-Savart relation for the induced velocity due to a vortex; he ascribes the method to Betz.

The treatment of one-dimensional flow which is the basis of the theory of compressible flow and of rocket propulsion is very well done. The chapter on viscosity deals only with viscous flow in tubes and with dimensional analysis of fluid flow. Here, on the other hand, instead of straightforward plotting of drag coefficient against Reynolds number or its logarithm, the author uses methods of plotting which seem confusing to the reviewer.

The book is of great interest to those engaged in teaching aeronautics and it should often be consulted by them to compare the clarity of their own presentation with that achieved by Professor Rauscher. Owing to the high price the book is unlikely to be bought by students, except perhaps by those working in isolation for whom the detailed exposition will be welcome.—H. B. SQUIRE.

QUEST FOR A NORTHERN AIR ROUTE. *Alexander Forbes. Harvard University Press 1953. Geoffrey Cumberlege, London 1954. 138 pp. Illustrated. 32s. net.*

Had I been an American writing "Quest for a Northern Air Route," I should have started with well justified pride by waving the Stars and Stripes over the achievements of American airmen in pioneering the idea of Arctic Air Routes; to begin with, the Army Fliers' round-the-world flight which took them through Iceland and Greenland to North America in 1924, miraculously losing only one machine out of three, and then of course the Lindbergh Arctic Survey flights for Pan American in 1933. This omission of Dr. Forbes is surprising if not regrettable.

For thirty-two shillings one is entitled to expect a book with first-class binding, photographs and subject matter, but "Quest for a Northern Air Route" certainly disappoints in the first two. The photographs appear in bunches of as many as four double-sided plates at a time and their quality both in imaginativeness of make-up and in finish leaves something to be desired.

Dr. Forbes has a story of great American achievement to tell in the survey and charting of Ungava and Frobisher Bays, when war-time conditions necessitated break-neck speed in the site-selection and construction of aerodromes. Considering all the difficulties, it is really surprising that there were few such major blunders as the siting of Bluie West One (for which Dr. Forbes had no responsibility) in E. Greenland. The British reader may find difficulty in adapting himself, as I did, to Dr. Forbes' style. I was reminded of Scott's simple words when he wrote from the tent in which he knew he was to die, "We are pegging out in a very comfortless spot . . ." by Dr. Forbes' description "Sherm Wengerd and I are cooped up in our little trapper's tent-for-two, with an icy gale shaking it as a terrier shakes a rat and the rain beating on its walls like machine-gun fire."

Flowery language unfortunately does not give colour to a flat story, and this story is flat apart from a very few breaks, like the Boston carpenter's intriguing vision, of the demise of Atlantic fliers Nungesser and Coli at a desolate spot on Meta Incognita in Baffin Land.

"Quest for a Northern Air Route" interested me a great deal, because I have had a personal connection with the problem, and because Baffin Land and the Arctic appeal to me. But I doubt very much whether readers who are making this their first acquaintance with the development of Arctic flying, would find it an easy and absorbing story to read.—JOHN GRIERSON.

THE AIRCRAFT OF THE WORLD. *William Green and Gerald Pollinger. Macdonald, London 1953. 160 pp. 25s. net.*

This work is like a miniature *Jane's* combined with an all-embracing spotters handbook. Some 330 aircraft are each treated to excellent 3-view silhouettes, one photograph and good brief factual information. Another 380 aircraft are treated as "also-rans," and included in each section with just a photograph and a few facts—unfortunately no spans are given.

Now the production and silhouette drawings in this book are admirable, but the authors—both of whom are in the recognition world—have apparently let the publishers nearly ruin the book for them. Everyone in the "game" knows that the best kind of photograph to give the overall character of an aircraft is a three-quarter front view, taken either from above or below the machine in question. Yet here every large half-tone block is 4 in. × 1½ in., which means of course that every machine has to be shown in almost side-view shots, many of which are precious little more use than the accompanying side-view silhouette. It also means the inclusion of far too many ground views. It is inconceivable to this reviewer that such an illustration policy could have been adopted. A nearly square block area is essential if a uniform proportion *has* to be adopted; but uniformity of block proportion should in any case be avoided in a book like this.

The other point on which the authors are courting danger is their classification, especially where jets are concerned. They divide single from twin jet aircraft and bring upon themselves all the confusion of the single jet with twin intakes, ditto with one intake, twin jets with twin intakes, and so on.

But there is no doubt of this book's usefulness; and it is probably true (as the publishers "blurb" states) that it illustrates all the aircraft that are to be seen in the air today. There is also an excellent index, just where an index for this sort of book should be, *in the front.*—C. H. GIBBS-SMITH.

THE COMPLETE AIR NAVIGATOR. *D. C. T. Bennett. Sixth Edition. Pitmans, London 1954. 444 pp. Diagrams. 30s. net.*

In his preface, Mr. Bennett states that "progress in Air Navigation in the last few years has been far from exciting," and so it is not surprising to find that this Sixth Edition is in many ways similar to its predecessor.

The main revisions are in the Appendices which show specimen pages from the new Air Almanac, together with the amended Flight Navigator's Licence requirements and syllabus. Included as a new feature are the Aircraft Performance Graphs which are used in the official licence examinations. This is a very good addition, but I believe the average reader would benefit if more guidance were given in their application.

It is strange to find in such an authoritative work, in this edition as in the fifth, that "true course may still be converted to magnetic by applying deviation." If this is a printer's error I dare say the author will have something to say to his publishers.

It is also a little difficult to understand why, following the introduction to Napier's Analogies, the student is introduced to a practical problem, the first step of which is solved using them and the second and final step using the much longer Haversine formula. However, these are but trivialities when one considers the comprehensive range and otherwise peerless presentation of the subject matter.

The preface of this work, which I feel still remains the classic of its field, suggests that some readers may well

be concerned in space navigation before they die. If so, it is hoped that the necessary revisions and additional chapters will be completed by the same masterly hand.—J. W. G. JAMES.

THE DYNAMICS AND THERMODYNAMICS OF COMPRESSIBLE FLUID FLOW. *Volume I. Ascher H. Shapiro. 647 pp. Index, figures. $16. Set $30.*

This is the first of two volumes; the second volume is due to be published in the spring of 1954. The author has set out to write a comprehensive account of compressible fluid flow, with emphasis on clear explanation and discussion of the fundamental physical principles. The presentation and choice of subject matter should give the book a strong appeal to engineers. Whenever possible working formulæ are derived from the theoretical results, and there are numerous comparisons with experiments. At the same time, many theoretical methods of little direct use to the engineer are rightly included, because they provide valuable information about the behaviour of fluids in certain typical situations. Most of the mathematics in this first volume is not difficult, and should be easily understood by any engineer who would use the book.

The first volume is divided into five parts. Part 1 starts with a discussion of some fundamental principles of fluid mechanics, and a careful explanation of the uses of systems and control volumes. This is followed by a full discussion of the first and second laws of thermodynamics and an account of the properties of a perfect gas. The velocity of sound, Mach cone, Mach number, and Mach angle are then introduced. A brief summary of optical methods of investigation concludes Part 1.

The second part contains a full account of one-dimensional flow, including the effects of friction, heat addition, and other complications. The analysis is made as general as possible; for example, it is not assumed at the beginning that the fluid is a perfect gas. To simplify the presentation, the first chapter considers only area changes in isentropic flow, deferring complications such as friction and heat addition until later. The next chapter deals with normal shock waves, considering first the general fluid, using the concepts of the Fanno and Rayleigh lines. The equations are then developed for a perfect gas, working formulæ are derived, and a number of examples of shock-wave formation are discussed.

Flow in constant-area ducts with friction is then considered, again starting with a discussion of the general fluid based on the Fanno line. The equations for a perfect gas are then developed for adiabatic and for isothermal flow, and working formulæ and charts are derived. Choking due to friction is fully explained. The next chapter deals with frictionless flow in constant-area ducts with heating or cooling. Moisture condensation shocks and explosion waves are also discussed. The final chapter in the part dealing with one-dimensional flow is a general treatment of the subject, with a large number of complicating factors included, using the approach given in the paper by Shapiro and Hawthorne (*J. App. Mech.*, 1947). The reader has been well prepared, however, for this rather formidable last chapter, by the gradual introduction of the complications in the earlier chapters.

In Part 3, the equations of motion for steady irrotational flow are derived, and the concepts of circulation, vorticity, velocity potential, and stream function are introduced and explained. This part consists of only one chapter, and serves as an introduction to the later chapters dealing with flow in two and three dimensions.

Part 4 starts with an excellent chapter on the linear approximation to two-dimensional subsonic flow. The approximations that are made in linearising the equation are explained very carefully, and the limitations of the theory are made quite clear. There is a full explanation of Gothert's similarity rule, and from this the simple Prandtl-Glauert rule is derived. The next chapter deals with the hodograph equations, used in conjunction with the "tangent-gas" approximation to derive the Kármán-Tsien pressure correction formula. This is followed by an account of the methods for calculating two-dimensional subsonic flow used by Janzen and Rayleigh and by Kaplan. There is a section on results of experiments on two-dimensional aerofoils at subsonic speeds; this is rather poor and contains some misleading statements about variations of drag coefficient.

The last chapter of Part 4 deals with Gothert's rule for three-dimensional subsonic flow. Only a brief explanation is given here, since the corresponding rule for two-dimensional flow is discussed fully in an earlier chapter. The application of Gothert's rule to bodies of revolution and to wings of finite span is considered. The account that is given of swept-back wings appears rather out of date at the present time; it is almost a summary of the state of knowledge in Germany in 1945.

Part 5 deals with two-dimensional supersonic flow, starting with an account of the linear approximation. The discussion of this relies rather too much on the previously described subsonic theory, and the limitations of the linearised supersonic theory are not explained very clearly. The linearised theory is applied to the calculation of supersonic flow past two-dimensional aerofoils, and some simple relations for force and moment coefficients are derived. Properties of Mach waves are also considered in this chapter. In the next chapter the method of characteristics is described for two-dimensional flow, using a physical approach based on the propagation of small disturbances along Mach lines. This is followed by a mathematical approach to the subject, to show that the method previously explained has a sound mathematical basis. (Some further remarks on the mathematical theory are given in an appendix.) The application of the method of characteristics to the design of nozzles and other problems is explained.

The last chapter in this volume is entitled "Oblique Shocks," but in fact this chapter covers a wider field than the title indicates and is of great interest. The conventional theory of oblique shock waves is given first, including the derivation of working formulæ and charts. This is followed by a general discussion of oblique shock waves and their interaction with Mach waves, the properties of very weak shock waves, strong and weak shock waves to produce a given deflection, and reflection and interaction of shock waves. The "shock-expansion" theory of two-dimensional aerofoils is explained (rather too briefly) and there is a satisfactory account of Busemann's second-order theory. Finally, there is a brief account of the interaction of shock-waves with boundary layers.

The list of contents of Volume II shows that this will deal with three-dimensional supersonic flow, hypersonic and transonic flow, unsteady one-dimensional flow, and laminar and turbulent boundary layers.

The arrangement of the book is very good; the subject is introduced in a logical order so that it can be easily followed. Numerous examples are given at the ends of the chapters. It is satisfactory to note that the book is based on a system of units in which unit force gives unit acceleration to unit mass (*e.g.* the slug, pound-force system). A minor criticism is that some of the references

of German origin are given in the form of American papers that are not generally available outside the U.S.A., although much of the material has in fact been published in Germany. The method of acknowledging the origin of schlieren and shadow photographs is rather unsatisfactory; the author's name is given but there is no indication of the paper or book in which the original photograph can be found.

Nevertheless, these are minor criticisms; the book on the whole is excellent, and will certainly be very useful to all engineers who have to deal with the flow of gases at high speeds. The only serious objection is the high price, more than £5 for each volume.—W. A. MAIR.

AERODYNAMICS OF PROPULSION. *D. Küchemann and J. Weber. McGraw Hill, London 1953. 340 pp. 64s. 6d. net.*

(The following (dated 11th April 1954) has been received from Mr. Annand.—Ed.)

A review of this book, prepared by the writer, appeared in the November 1953 Journal (p. 732). The authors have, in correspondence, pointed out three inaccuracies in this review, which I should like to correct.

(*i*) I stated: "The discussion of air-air intercoolers on pp. 295-7 appears to be based on incorrect equations." It should be pointed out that the equations are a correct representation of the assumptions, and that it is the latter with which I disagree, although I am informed that they are commonly accepted, no others having been developed.

(*ii*) I remarked: "There is a discussion of the relative effect of inlet and exhaust pipe losses on turbo-jet thrust which is based on the assumption of constant-volume-flow operation, which is unlikely to occur in practice." This is inexact. The discussion concerns a comparison of the outputs of separate engines, operating at the same volume flow, with and without inlet and exhaust losses. It is not intended as a consideration of the behaviour of a particular engine installed in various ways. Whether the discussion has practical relevance (the point raised in the comment) remains debatable.

(*iii*) One comment, that the reader might be left with the impression that diffuser losses depend only on area ratio, cannot be sustained, and I withdraw it without reservation. —W. J. D. ANNAND.

TEXTBOOK OF THE MATERIALS OF ENGINEERING. *Herbert F. Moore and Mark B. Moore. McGraw Hill Book Co., 1953. Eighth Edition. 363 pages. 48s. net.*

In this new edition information not available at the time of the last edition has been incorporated, and special chapters on cohesion stress and strain, on concrete, and on plastics by experts in these fields have been added.

The book is intended primarily for students in technical schools. The authors hope that it will be useful to draughtsmen, inspectors, machinists and others who deal with materials of engineering in their work. They have dealt with their subjects in a fairly elementary way.

After a chapter giving a general introduction to the subject, others follow dealing with cohesion stress and strain; elastic strength of materials; failure by flow and creep; failure by fracture under static and dynamic stresses; physical properties of structural metals; structural damage, factor of safety, working stress; failure by corrosion and wear. Later chapters deal with production of the common structural metals, seven pages being taken up on iron and steel production; short notes on the non-ferrous metals and castings follow. Wood, building stone and ceramics, cement and concrete, plastics, rubber leather rope, and testing and inspection work are dealt with next. The concluding chapters deal with specifications for materials, with examples and a note on structural damage to stressed materials.

The work is presented in very readable style. Two pages are taken up with the nomenclature of fatigue testing. Addition of similar explanations to the next edition of the terms of stress and strain with more assistance to the student on items such as rigidity and Poisson's ratio would seem to be worth while.

Each chapter is followed by selected references for further study and questions suitable for exercises. Name and subject indexes are appended.—H.S.

DIMENSIONAL METHODS AND THEIR APPLICATIONS. *C. M. Focken. Edward Arnold, London 1953. 220 pp. 30s. net.*

It would appear that a "dimensional" scaffolding is implicitly embodied in the "build up" of the physical aspect of natural philosophy.

Although the basic idea of dimensions of physical quantities is almost intuitive to the mathematical physicist the formulation of an "algebra" of dimensions has been beset with acute controversy on the very elements of the subject. To quote from the author's preface:

"One authority said, a few years ago, that almost every proposition about dimensions asserted by one writer, has been denied by a writer of equal competence."

Such notions as to whether dimensions apply to the actual quantities or to the units in which they are appraised or to both have become matters of lively speculation and dispute. For instance Focken states:

"The meaning of dimensions adopted makes it clear that they are *not* characteristic of the ultimate nature of the physical quantity considered. They characterise the method of measurement selected, and they express the dependence of the change in a derived magnitude upon changes in the arbitrarily chosen fundamental magnitudes."

He quotes from Clerk Maxwell's treatise on electricity and magnetism: "In treating the dimensions of units we shall call the unit of length $[L]$. If l is the numerical value of a length, it is understood to be expressed in terms of the concrete unit $[L]$, so that the actual length would be fully expressed by $l\,[L]$."

Concerning which Focken remarks:

"Maxwell uses the same symbol, however, to denote the dimensions of units, thus he writes $[L]$ for the dimension of the fundamental unit of length, and $[LT^{-1}]$ for the dimensions of the derived unit of velocity."

Again Focken says regarding a discussion on the subject in the *Philosophical Magazine*:

"To Campbell's contention that we could never be certain that the dimensional treatment of any problem was correct unless the differential equations had actually been written down, Buckingham replied that we are never absolutely sure of anything in physics until it has been proved by experiment, even if we have obtained the differential equation."

The *raison d'etre* of a work such as the one under review is thus fairly evident. The book is a comprehensive survey of value and of interest and is likely to make a wide appeal, touching as it does practically every branch of physical science.

The text is arranged in five chapters:

1. Elementary treatment of dimensions. 2. Dimensional analysis or the method of dimensions. 3. Dimensions of various physical magnitudes: difficulties and criticisms. 4. Physical applications of dimensional methods. 5. Engineering applications of dimensional methods.

A useful feature is the inclusion of a series of problems in Chapters 4 and 5, to which the answers are also given.

In addition there is a bibliography and an index.

The publishers and their printers merit praise for the excellent set up and production of the book.—J. MORRIS.

COMPLEX VARIABLE THEORY AND TRANSFORM CALCULUS. *N. W. McLachlan. Cambridge University Press, Cambridge 1953. 388 pp. Illustrated. Index. 55s. net.*

This is the second edition of the book which was first published in 1939 and it is intended primarily for mathematical technologists rather than for pure mathematicians.

The first part is devoted to the theory of complex variables and covers the theorems of Cauchy, Taylor and Laurent on integration. This subject is covered more fully than in the previous edition, especially integration round branch points.

Part II on the theory of transform calculus deals with the Mellin inversion theorem, solution of ordinary linear differential equations, impulses and frequency spectra, the proofs of the more important theorems being given in the appendices at the end of the book. Partial differential equations are dealt with briefly in Part III.

Part III is devoted chiefly to worked out examples based on the theory of the first two parts; these examples should be of value to anyone interested in electrical circuits, radio television and in submarine cables. There are also a number of examples left to be worked out by the reader.

The standard of the illustrations is high, although in a number of cases an important diagram has been printed several pages away from the text to which it refers.—M.M.

THE AEROPLANE DIRECTORY. *1954 Edition. Compiled by the Staff of The Aeroplane. Temple Press Ltd. 546 pp. 21s. net.*

Although this year's Directory costs 6s. more than that for 1953, and there are but 6 more pages, the improvements amply justify the higher price. For one thing, the various sections now have tab indicators. These will save more than the extra cost in man-hours alone.

New sections appear on N.A.T.O. air personnel and, of more esoteric interest, lists of Past Presidents of the Society and of Past Chairmen and Presidents of the Society of British Aircraft Constructors. A really useful addition is a list of trade names of aeronautical and allied firms.

Would it be useful to allow space for users to make manuscript additions and adjustments during the year?—F.H.S.

TABLES OF COEFFICIENTS FOR THE NUMERICAL CALCULATION OF LAPLACE TRANSFORMS. *Editor, H. E. Salzer. U.S. Government Printing Office, Washington 1953. 36 pp. 25 cents net.*

The tables in this book are designed to facilitate the numerical evaluation of infinite integrals expressible in the form of Laplace transforms, such as arise in theory of heat conduction and in various branches of electrical engineering.

A short Introduction explains the use of the tables, the method of computing them, and gives examples of their application.

Table I gives the coefficients A_i for the 2-point through 11-point cases, and in every case the argument does not range beyond the largest value of t at which $f(t)$ is given.

Table II gives evaluations of $(n!)/(p^n+1)$ which is useful for obtaining the Laplace transform of a polynomial when its coefficients are given instead of its values at certain points.

There are also two Schedules giving the Lagrange interpolation coefficients and the Laplace transforms of interpolation coefficients.—M.M.

Additions to the Library

Bijlaard, P. P. and G. S. Johnston. COMPRESSIVE BUCKLING OF PLATES DUE TO FORCED CRIPPLING OF STIFFENERS: PARTS I AND II. Sherman Fairchild Paper 408. I.A.S. 1953.

Blanc, E. L'AVIATION DES TEMPS MODERNES. Larousse. 1953.

C.A.A. AIRCRAFT DESIGN THROUGH SERVICE EXPERIENCE. (C.A.A. Technical Manual No. 103.) U.S.G.P.O. 1953.

*Camm, F. J. (*Editor*). NEWNES ENGINEER'S REFERENCE BOOK (6th edition). Newnes. 1954.

*Cescotti, R. AVIATION DICTIONARY. German-English. English-German. Hanns Reich. 1954.

Chapel, C. E. JET AIRCRAFT SIMPLIFIED. 2nd edition. Aero Publishers Inc. 1954.

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Eck, B. TECHNISCHE STROMUNSLEHRE. 4th edition. Springer. 1954.

Esclangon, E. L'ACOUSTIQUE DES CANONS ET DES PROJECTILES. Imprimerie Nationale. 1925.

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Houwink, R. ELASTICITY, PLASTICITY AND STRUCTURE OF MATTER. 2nd Edition. C.U.P. 1954.

I.A.S. THEORY AND PRACTICE OF SANDWICH CONSTRUCTION IN AIRCRAFT. I.A.S. 1948.

Jensen, H. M. (*Editor*). ILLUSTRATED DESIGN MANUAL FOR THE REPAIR OF ALUMINIUM ALLOY STRUCTURES. Johnson Res. Corp. 1952.

Logan, J. (*Editor*). THE COMPLETE BOOK OF OUTER SPACE. Sidgwick and Jackson. 1953.

Manchester Joint Research Council. INDUSTRY AND SCIENCE. Manchester University Press. 1954.

*McFarland, M. W. (*Editor*). THE PAPERS OF WILBUR AND ORVILLE WRIGHT. Vol. I. 1899-1905. Vol. II 1906-1948. McGraw Hill. 1953.

National Bureau Standards. CHARACTERISTICS AND APPLICATIONS OF RESISTANCE STRAIN GAUGES. U.S.G.P.O. 1954.

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Peterson, R. E. STRESS CONCENTRATION DESIGN FACTORS. Wiley. 1953.

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Sutton, O. G. MATHEMATICS IN ACTION. Bell. 1954.

Timoshenko, S. P. COLLECTED PAPERS. McGraw Hill. 1953.

Tin Research Institute. THE PROPERTIES OF TIN. Tin Research Institute. 1954.

Wright, Orville (Introduction and commentary by F. C. Kelly). HOW WE INVENTED THE AEROPLANE. David McKay Co. 1953.

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Reports

AERODYNAMICS

Boundary Layer

See also Testing and Instruments

A review and assessment of various formulæ for turbulent skin friction in compressible flow. R. J. Monaghan. A.R.C. Current Paper No. 142 (*October* 1953).

Despite a lack of experimental evidence, numerous formulæ have been developed for the variation of turbulent skin friction on a flat plate in compressible flow, with and without heat transfer. The present note makes an extended comparison of available formulæ and examines the assumptions made in their development, checking against experimental evidence where possible.—(1.1.3).

Fluid Dynamics

A new relaxational treatment of the compressible two-dimensional flow about an aerofoil with circulation. L. C. Woods and A. Thom. R. & M. 2727 (*March* 1950, *published* 1953).

The incompressible two-dimensional flow about an aerofoil with circulation is calculated using relaxation on the square mesh formed by the incompressible velocity equipotentials (ϕ=constant) and the streamlines (ψ=constant). Log (l/q_o) and θ_o, where (q_o, θ_o) is the incompressible velocity vector on polar co-ordinates, are harmonic functions in the (ϕ, ψ)-plane, and can be found by well-known relaxation or squaring methods. Boundary conditions are specified in the (x, y) or physical plane, but starting from an assumption for the surface velocity, approximate boundary conditions can be found for the (ϕ, ψ)-plane, which then enable a more accurate value of the surface velocity to be calculated, and so on. The circulation is imposed on the field by having a smaller number of equipotential lines of the mesh cutting the lower surface of the aerofoil than cutting the upper surface.—(1.4.1 × 1.10.1.1).

Two-dimensional wind-tunnel interference. L. G. Whitehead. R. & M. 2802 (*June* 1950, *published* 1954).

Exact solutions are given for the inviscid flow past two cylindrical profiles in the centre of a stream of limited depth. The first of these relates to a nearly circular cylinder and the second to a thin section giving a constant pressure drop over the greater part of its surface. The stream has either parallel walls, constant pressure walls, or the boundaries may be partly of the constant pressure type. For the thin profiles the changes of thickness ratio required to give the same pressure distribution as in an unlimited flow are found.—(1.4.1 × 1.5.1.4).

Calculation of the influence of internal circulation in a liquid drop on heat transfer and drag. W. R. Conkie and P. Savic. N.R.C., Canada, Report No. MT-23 (*October* 1953).

By means of a mathematical artifice from the theory of slip flow it is demonstrated that, if circulation exists in a liquid drop as a result of its motion through a viscous medium, the boundary layer is reduced by a factor depending on the ratio of viscosities inside and outside the drop. It follows that the film heat transfer coefficient may be materially increased, a conclusion which receives some support from some recent experimental data.—(1.4).

Mesures statistiques de la corrélation dans le temps. A. Favre. O.N.E.R.A. Publication 67 (1953).—(1.4.2).

Incompressible flow past a sinusoidal wall of finite amplitude. Carl Kaplan. N.A.C.A. Technical Note 3069 (*February* 1954).

Plane incompressible flow past an infinitely long sinusoidal wall of any amplitude is considered in the present paper. It was found that this problem could not be treated in the physical plane but had to be transferred to the plane of velocity potential and stream function. In this plane, the problem was not only successfully treated by the small disturbance iteration method but, moreover, its solution was rigorously expressed in the form of a non-linear integral equation.—(1.4.1).

Internal Flow

See also Fluid Dynamics

Report on the flow phenomena at supersonic speed in the neighbourhood of the entry of a propulsive duct. G. H. Lean. R. & M. 2827 (*March* 1945, *published* 1954).—(1.5.1).

Elimination of flow instability in two high-speed wind tunnels by means of high-drag screens. J. Lukasiewicz. N.A.E. Canada, Laboratory Report LR-83 (*November* 1953).

Pitot-tube boundary layer traverses and force measurements on half-models made in two trans- and supersonic wind tunnels have shown large amplitude flow fluctuations near the working section walls. The flow instabilities were effectively eliminated by installation of high-drag screens (giving a pressure drop of 100 to 200 velocity heads) at the contractions' entries.—(1.5.1.4).

Study of three-dimensional internal flow distribution based on measurements in a 48-inch radial-inlet centrifugal impeller. J. T. Hamrick, J. Mizisin, and D. J. Michel. N.A.C.A. Technical Note 3101 (*February* 1954).

A study of the loss and velocity distribution in a radial flow impeller was made. It is indicated that secondary flows within the boundary layer and leakage through the blade-to-shroud clearance space result in a concentration of low-energy air at approximately 80 per cent. of the passage width from the pressure face at the shroud.—(1.5.1.2).

Stability and Control

Effects of wing position and fuselage size on the low-speed static and rolling stability characteristics of a delta-wing model. A. Goodman and D. F. Thomas, Jr. N.A.C.A. Technical Note 3063 (*February* 1954).

Results are presented of an investigation made to determine the effects of wing position and fuselage size on the low-speed static and rolling stability characteristics of models having a triangular wing and vertical tail surfaces. Interference increments between the various components are evaluated, and the variation of the vertical-tail lift-curve slopes and the efficiency factors with angle of attack as affected by wing position and body size are presented. Tuft-grid pictures of the flow at the vertical tail as affected by wing-fuselage interference are also presented.—(1.8.0).

Comparison of model and full-scale spin recoveries obtained by use of rockets. S. M. Burk, Jr., and F. M. Healy. N.A.C.A. Technical Note 3068 (*February* 1954).

An investigation of a 1/19-scale model of an unswept-wing trainer aeroplane was conducted in the Langley 20-foot free-spinning tunnel to determine the rocket spin-recovery characteristics of the model for comparison with available full-scale-aeroplane results. A rocket was attached to each wing tip to fire in a direction to apply an anti-spin yawing moment about the Z body axis. The rockets were fired individually and in combination.—(1.8.3).

A simple mechanical analogue for studying the dynamic stability of aircraft having nonlinear moment characteristics. T. N. Canning. N.A.C.A. Technical Note 3125 (*February* 1954).

The analogy between a ball rolling on a suitably contoured surface and a pitching and yawing missile in free flight is developed. The analogue is checked experimentally for the case of linear moment characteristics. Several non-linear cases are also treated experimentally. Results of ballistic-range firings are also included.—(1.8.0).

Wings and Aerofoils

See also Fluid Dynamics

Pressure distribution and boundary layer investigations on 44 *degree swept-back tapered wing. Part I—Three-dimensional*

NOTE:—*The figures in parentheses at the end of each Summary are for office use only.*

tests on the wing. Part II—Two-dimensional pressure distribution tests on 10 *per cent thick symmetrical aerofoil section. J. Black. A.R.C. Current Paper No.* 137 (*August* 1953).—(1.10.2.1 × 1.10.2.2).

Wind-tunnel tests on two-dimensional supersonic aerofoils at $M = 1.86$ *and* $M = 2.48$. *D. Beastall and R. J. Pallant. R. & M.* 2800 (*July* 1950, *published* 1954).

Wind-tunnel tests, at Mach numbers 1·86 and 2·48, have been carried out on two-dimensional upswept double-wedge and circular arc aerofoils to study the viscous effects which are not accounted for in the linearised and shock-expansion aerofoil theories. The aerofoil characteristics derived from the measured surface pressures are compared with the theoretical values. Schlieren observation was employed to examine the flow and, in particular, the separation near the trailing edges of the aerofoils. In an appendix the results obtained from experiments on breakaway caused by a step on a flat plate, are applied to the aerofoil tests as a method of assessing the pressures in the dead-water regions formed by the flow separation, and comparison is made with the measured pressures.—(1.10.2.1).

Low speed wind tunnel tests of a 3′ *chord two-dimensional Vampire tip section using suction through discrete holes near the leading edge. P. B. Atkins and R. S. Trayford. A.R.L. Australia, Flight Note* 21 (*November* 1953).

Low speed wind tunnel tests to determine the order of maximum lift coefficient obtainable on a 10·6 per cent. thick wing have shown discrete suction holes to be a most promising method of controlling the boundary layer near the leading edge.—(1.10.2.1).

The oscillating wing of low aspect ratio—results and tables of auxiliary functions. H. Merbt and M. Landahl. K.T.H. Sweden, Aero T.N. 31 (*January* 1954).

The theory of oscillating slender wings, given in T.N. 30 has been applied to the case of a delta wing oscillating in plunging, pitching, rolling, bending and torsional motion. The expressions for the flutter derivatives and pressure distributions are summarised and the auxiliary functions involved are tabulated.—(1.10.1.2 × 2.0).

Testing and Instruments

Chemical solids as diffusible coating films for visual indications of boundary-layer transition in air and water. J. D. Main-Smith. R. & M. 2755 (*February* 1950, *published* 1954).

Experimental investigations have been made on various chemical solids as diffusible coating films for visual indication of boundary-layer transition in air and water. Originally, the method was applicable only at low speeds in wind tunnels and water tanks, and the indications were somewhat transient. More durable coating materials have now been made available, admitting of use at subsonic and supersonic wind-tunnel speeds from 30 to 1,350 m.p.h. and at ship-hull speeds from 2½ to 20 kts. The method has also proved capable of extension to aircraft in flight at speeds from 100 to 445 m.p.h. at temperatures down to −22 deg. C. and at altitudes up to 20,000 ft. The diffusible-solid-coating method, with its advantages of autographic indication and simplicity and rapidity of operation has thus become a versatile technique in investigations on fluid flow in aerodynamics and hydrodynamics.—(1.12 × 1.1.2 × 17.0).

*Wind tunnel balance for half-model tests. J. Ruptash and E. K. Parks. N.A.E. Canada, Laboratory Report LR-*82 (*November* 1953).

A three-component half-model, strain gauge, external-type balance, incorporating a double-pivot system, has been designed for the National Aeronautical Establishment 16-by 30-inch intermittent flow, high-speed wind tunnel. The results of the calibration and preliminary tests are given.—(1.12.4).

An experimental method of determining the drag of a shock wave with application to a ducted body. L. Ohman. F.F.A. Sweden, Report 51 (1954).

A method for experimental determination of the drag caused by a shock wave has been analysed in some detail. Only the axially symmetrical case is treated. The method is based on a determination of the wake behind the shock wave and works from the shape of the shock obtained by the schlieren or shadowgraph method. The calculation of shock angle and the extrapolation of the photographed part of the shock to infinity has received special attention. As an example, a wind tunnel investigation with a ducted body at various inlet mass flow conditions is treated.—(1.12 × 1.2.3.2).

AEROELASTICITY

See also Aerodynamics: Wings and Aerofoils

Estimation of the effects of distortion on longitudinal stability of swept wing aircraft at high speeds (sub-critical Mach numbers). B. S. Campion. College of Aeronautics Report 77. (*January* 1954).

The effects of distortion on the longitudinal stability of swept wing aircraft at high speeds (sub-critical Mach numbers) are considered on a quasi-static basis. The method employed is based on the theory of Gates and Lyon but involves some extension of this theory. The analysis is illustrated by means of a simple example of a swept wing fighter aircraft for which wing, fuselage and tail distortion effects are considered, and the results are discussed with reference to the individual and combined distortion effects as well as the effect of compressibility.—(2).

Determination of reversal speed of a wing with a partial-span flap and inset aileron. W. G. Molyneux and E. G. Broadbent. R. & M. 2793 (*February* 1950, *published* 1954).

Control reversal due to deformation of a wing with a partial-span flap and inset aileron is considered theoretically for the particular case of a flap held at the root end. The semi-rigid method is used and an investigation is made for a particular aircraft.—(2).

The flutter of swept and unswept wings with fixed-root conditions. Part I—Wind-tunnel experiments. Part II—Comparison of experiment and theory. Part III—Wing torsional stiffness criterion. W. G. Molyneux. R. & M. 2796 (*January* 1950, *published* 1954).

The "fixed-root" flexure-torsion flutter characteristics of four model wings of different taper ratios have been investigated in the wind tunnel. The wing inertia axis and the angle of sweepback have been varied on each wing over the ranges 0·4*c* to 0·5*c* and 0 deg. to 50 deg. respectively. The experimental results support the present wing torsional stiffness criterion. Simple amendments to the criterion are put forward for the effects of sweepback.—(2).

Représentation des vibrations libres d'une aile dans le vent a l'aide de la notion de mémoire. R. Mazet. O.N.E.R.A. Note Technique 1954.—(2).

ELECTRONICS

An electronic apparatus for automatic recording of the logarithmic decrement and frequency of oscillations in the audio and subaudio frequency range. C. O. Olsson and K. Orlik-Rückemann. F.F.A. Sweden, Report 52 (*February* 1954).

An electronic apparatus for automatic evaluation of the damping of a harmonic oscillation has been designed and constructed. The apparatus is based on the idea of representing the harmonic damped oscillation by a rotating vector on the screen of a cathode-ray tube in such a way that the rate of decrease of the length of the vector is a measure of the damping.—(11).

FLIGHT TESTING

A survey of performance reduction, with particular reference to turbo-propeller aircraft. K. J. Lush. R. & M. 2757 (*January* 1950, *published* 1954).

The purpose of performance reduction is briefly examined. Methods in use are classified into experimental methods, which require no advance numerical data, and analytical methods, which require such data. The latter class is sub-divided into methods based on small corrections and methods based on performance analyses. The suitability of each class of method is discussed.—(13.1).

An investigation into the pitot rake method of measuring turbo jet engine thrust in flight. J. Stephenson, R. T. Shields and D. W. Bottle. A.R.C. Current Paper No. 143 (1954).

Tests have been made to establish whether a pitot rake could be used as an absolute measure of the thrust of a jet engine on the ground and in flight. The tests were made to investigate errors due to the assumptions inherent in the single pitot method of estimating thrust in flight, and to establish if a rake can be used to calibrate the single pitot of an uncalibrated engine installed in an aircraft. The tests were also planned to check the generally accepted non-dimensional thrust relationship for jet engines.—(13.1 × 27.1).

MATERIALS

*Hot hardness testing of chromium base alloys. H. T. Greenaway. A.R.L. Australia, Report SM-*195 (*June* 1952).

A dynamic hardness tester for use up to 1,000°C. is described. It has been used to determine the hardness of selected chromium-titanium, chromium-tungsten and chromium-beryllium binary alloys at 750° and 900°C. for times up to 1,000 hours. For comparative purposes "Nimonic 80A" and pure chromium have also been tested.—(21.6.2).

Contribution a l'étude de la déformation plastique. B. Jaoul. Publications Scientifiques et Techniques du Ministère de L'Air, France, No. 290 (1954).—(21.2.0).

POWER PLANTS

See also FLIGHT TESTING

General performance calculations for gas turbine engines. D. H. Mallinson. R. & M. 2684 (*June* 1946, *published* 1954).

In this report an attempt is made to summarise the theoretical work carried out during the past few years aimed at discovering the potentialities of the gas turbine as a power plant in many fields of application, but especially as an aircraft power unit. To do this the performance of the various modifications of the ideal gas turbine cycle is considered in some detail, and the works of various authors are then combined and edited in order to depict the performance attainable by practical engines. The influence of component efficiencies on this latter performance is examined and the effects of modifications, such as reheating the gas after partial expansion or introducing a heat exchanger, are compared with the effects predictable from the ideal cycle calculations.—(27.1).

A brief review of the problem of exhaust silencing. F. L. West. R. & M. 2803 (*May* 1946, *published* 1954).

This note reviews past and present work on noise reduction of the reciprocating engine exhaust. Collected measurements of the noise level surrounding various engine installations and the effect of silencing experiments on engine and aircraft performance are presented. In view of the growing application of the gas turbine, some recent observations of its noise characteristics are included. The adverse effects of simple baffle silencers on engine power illustrate the need for renewed investigation of silencing by acoustical interference methods allowing unrestricted gas flow. In this respect, limitations of past work on the theory of silencers are discussed and possible improvements suggested.—(27.0).

SCIENCE—GENERAL

Essai sur la convection naturelle. P. Vernotte. Publications Scientifiques et Techniques du Ministère de L'Air, France, No. 288 (1954).—(32.2.2).

STRUCTURES

LOADS

Counting accelerometer results from a Bristol "Freighter" aircraft operating in south-eastern Australia. Queenie Baum and F. H. Hooke. A.R.L. Australia, Structures and Materials Note 207 (*August* 1953).

Flight load measurements have been made by means of a counting accelerometer installed in a Bristol "Freighter" aircraft. During the recording period, totalling 95 hours of operation in the autumn and winter months, the aircraft was flown on routes in the south-eastern part of Australia.—(33.1.2).

THEORY AND ANAYLISIS

Testing and analysis of a 60° swept back wing with ribs parallel to the line of flight. D. Howe. College of Aeronautics Report 66 (*August* 1953).

The specimen used in this work represents the load carrying structure of a cambered two spar 60° swept-back wing, having closely pitched ribs of large boom area parallel to the line of flight, and thick skins. The wing has an aspect ratio of 2·45, a semi-span of 101·5 in. and a taper ratio of 1·50. Construction was of light alloy, and the root was built into concrete. Loading was by bending and torsion couples, and normal shear forces applied at, or near to, the tip. Both strain and deflection measurements were made. The strains in portions of the structure removed from root effects, and the stiffnesses were compared with the oblique co-ordinate theory of Hemp.—(33.2.3.2).

Stress concentration in swept wing panels using photoelastic models. G. M. Coiley. College of Aeronautics, Report 78 (*March* 1954).

A theoretical solution for stress concentrations as might occur in a swept wing, has been derived by E. H. Mansfield. This thesis applies photoelastic techniques to models of wing panels with various angles of sweep, with the object of verifying these equations.—(33.2.3.2).

*The root section of a swept wing. A problem of plane elasticity. B. C. Hoskin and J. R. M. Radok. A.R.L. Australia, Report SM*219 (*November* 1953).

The methods of N. I. Muskhelishvili are used to obtain the stresses in an approximately square plate, subject to concentrated forces at two opposite corners, acting in the direction of the diagonal, and to certain shear stress distributions along the sides. These shear stress distributions have been chosen to conform approximately to those observed at the corresponding boundaries during tests on a 45° swept-back tube with ribs normal to the leading edge.—(33.2.3.2).

TESTING

Records of major strength tests. P. B. Walker. R. & M. 2790 (*July* 1949, *published* 1954).

The strength attained in major strength tests, made over a period of ten years, is given for twenty-four wing systems and ten fuselages. A preliminary analysis is also presented from the standpoints of safety and design efficiency.—(33.3.2).

THERMODYNAMICS

THERMODYNAMIC PROPERTIES

*Preliminary experimental investigation of howling in reheat combustion chambers. H. U. Wisniowski. N.A.E. Canada, Laboratory Report LR-*84 (*September* 1953).

In commencing this investigation, howling combustion was considered to be related to the phenomenon known in acoustics as "singing flames" and first experiments were made with a gas burner on pipes of several dimensions. Later, a small-scale rig (diameter 5 inches), simulating conditions in a turbo-jet engine with re-heat, was used.—(34.1.1).

*The flame stability limits of a baffle in a hot fast gas stream with kerosene fuel. N. Golitzine. N.A.E. Canada, Laboratory Report LR-*88 (*December* 1953).

Experiments showed that the flame stability limits of a baffle in a hot fast gas stream, similar to the gas stream in the jet pipe of a jet engine with exhaust re-heat, depend to a very large extent on the location of the fuel jets upstream of the baffle.—(34.1.1).

*Flame stabilization in a fast air stream by the upstream air blast injection of kerosene fuel. N. Golitzine. N.A.E. Canada, Laboratory Report LR-*86 (*December* 1953).

Experiments showed that flame stabilisation in air streams of over 200 ft./sec. velocity is possible without plate baffles by the upstream air blast injection of kerosine fuel. There is an optimum blast air/fuel ratio which gives maximum blow out velocity, and the limit is raised by an increase of fuel flow with a corresponding increase of blast air at this ratio.—(34.1.1).

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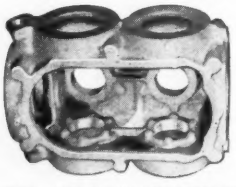

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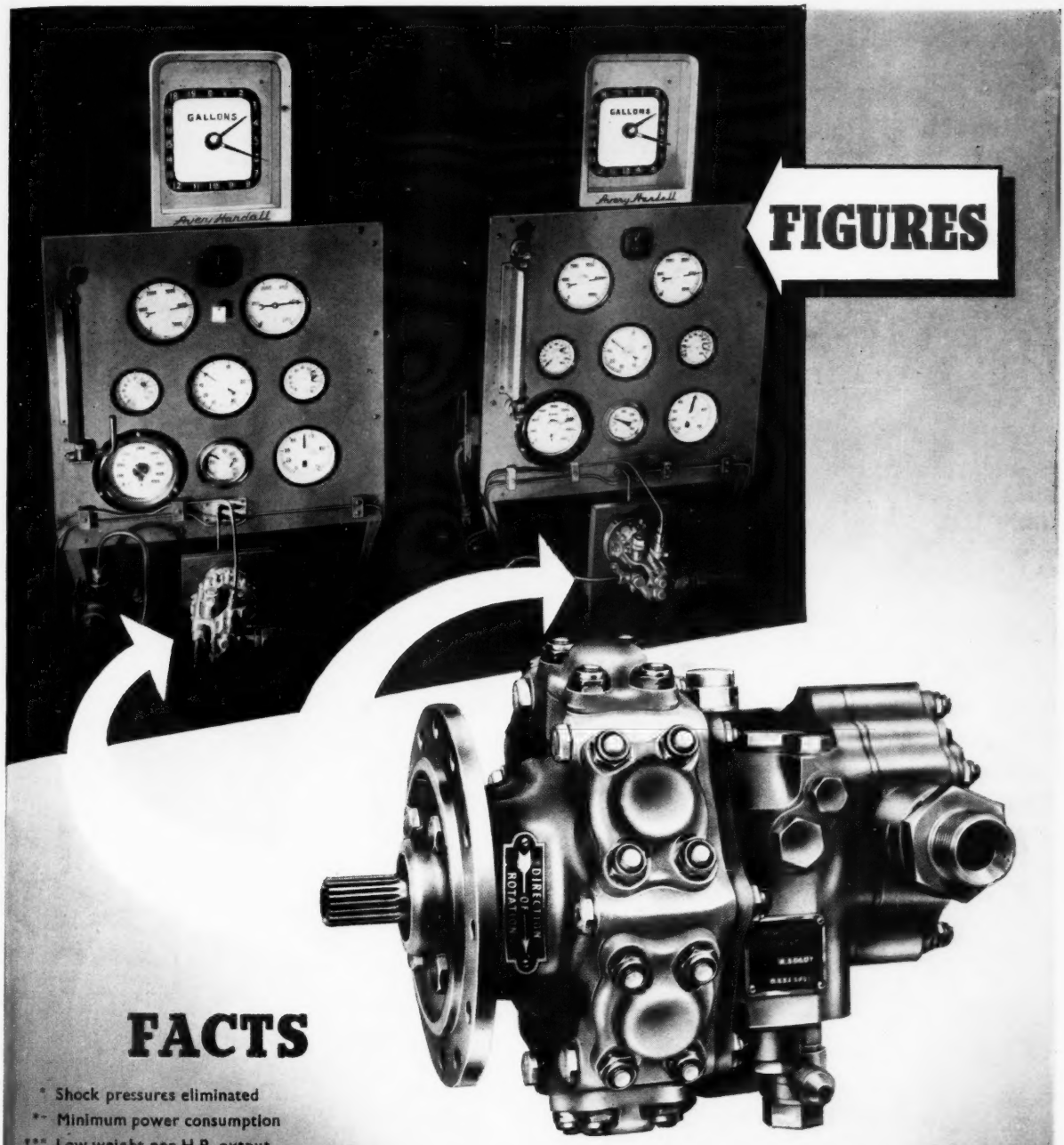

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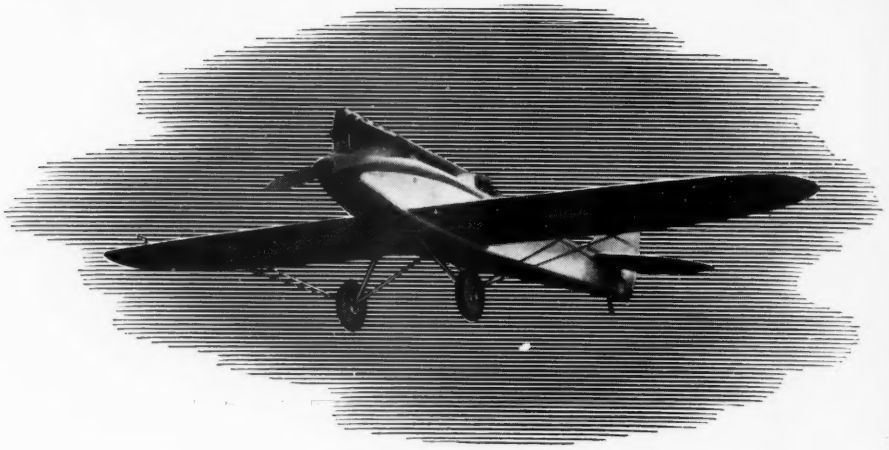

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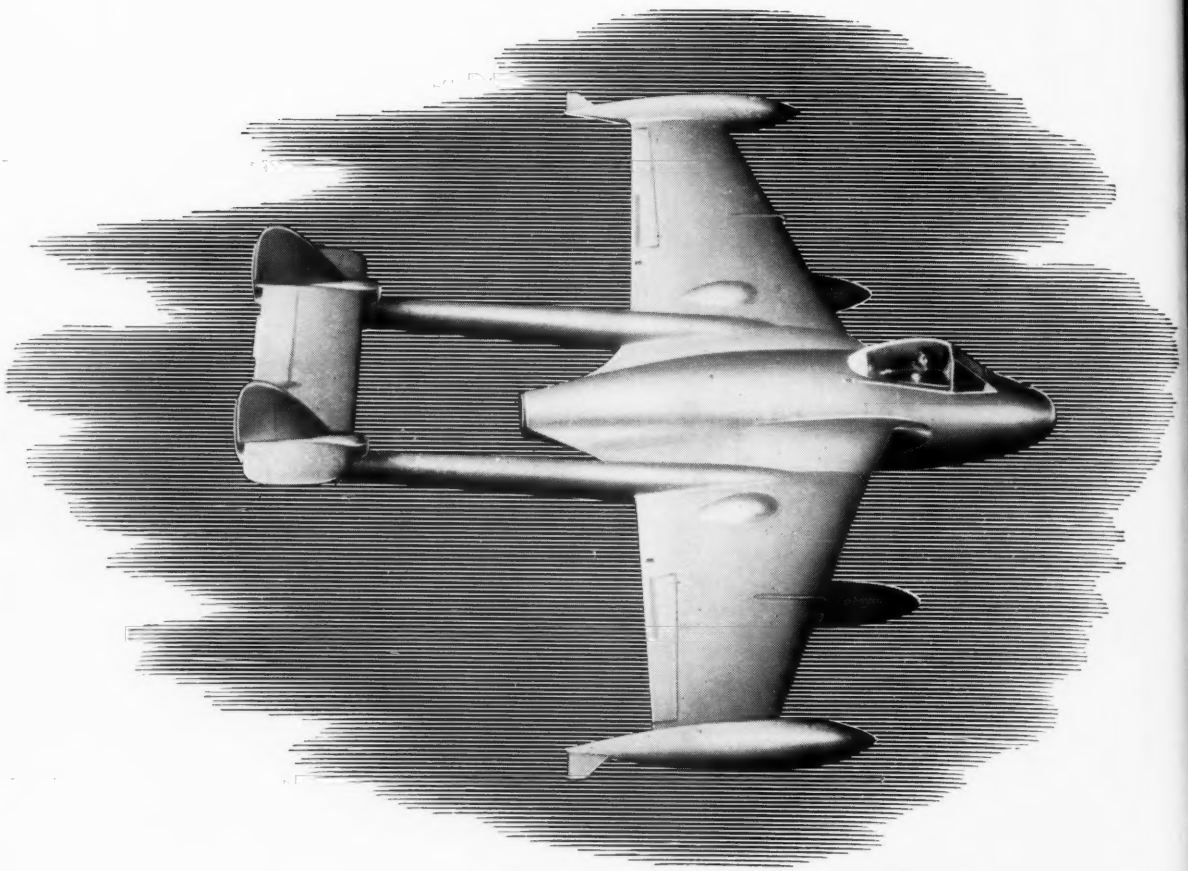

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